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Vol. V

TRANSCRIPT OF RECORD

NARRATIVE STATEMENT OF PATENT EVIDENCE

Supreme Court of the United States

OCTOBER TERM, 1930

No. 378

STANDARD OIL COMPANY (INDIANA), STANDARD OIL COMPANY (NEW JERSEY), STANDARD DEVELOPMENT COMPANY, ET AL., APPELLANTS,

vs.

THE UNITED STATES OF AMERICA

APPEAL FROM THE DISTRICT COURT OF THE UNITED STATES FOR THE NORTHERN DISTRICT OF ILLINOIS

FILED AUGUST 23, 1930

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VOL. V

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Q. And only one of those is set forth in the claims—that is, the one where there is no rising of the temperature?

A. There is not a specific statement in the claims, that the other is not covered. In fact, in Claim 1, the question of whether it is a lower, or a higher temperature, is not mentioned. It says "through an enlarged passageway"—[fol. 538] which is the chamber, I presume—"without increasing"—yes, it is,—"without increasing the temperature." I correct myself.

Q. In other words, that is in all the claims?

A. Is it? I haven't yet gone through all the claims. There are none of the claims that cover a rise of temperature, as the invention described in the specification covers.

Q. Well, that doesn't answer the question. They all specify that there is no increase, do they not?

A. Yes, sir, they all specify that there is no increase.

Q. Now, where do you find in the Pielsticker patent, any statement that there is no increase in temperature in the drum? And, by drum, I assume that we will agree that the analog is a part which Pielsticker calls the retort, L?

A. The retort is L.

The Master: And in the Ellis patent, L is a heating tank?

The Witness: No—it is a passageway.

The Master: I was asking about the Ellis patent.

The Witness: It is the figure 8.

The Master: But the analog is a drum, or heating tank?

The Witness: Yes.

Q. In other words, just to make this clear—your position is that part 8, of Ellis, corresponds to the retort, L, of Pielsticker—is that true?

A. That is correct.

Q. Now, where do you find in Pielsticker's patent, that the temperature is not raised in the retort, L, or drum?

The Master: You know this is a sort of an argument carried on. Obviously counsel, in putting these patents before the Court—the Court is just as able to read them, as Dr. McKee is, and this line of argument, and the time taken to search through, by the Doctor, will stretch this hearing out to nobody knows when.

Mr. Wiles: May I say this, on that matter, if the Court please—in the first place, as cross examination, when the

witness has said that this has got it all, and I can't find it, I certainly have the right to ask him where he finds it. Now, he has stated expressly that the Pielsticker patent met the Ellis patent, and had it all, and I can't find it there, and I am entitled to ask him where he found it.

The Master: You can put your finger on the element in the Ellis patent, which you say is not in the Pielsticker patent, and if he says he can find it, let him point it out—where it is.

[fol. 539] Mr. Wiles: That is just what I am after. Where does he find it? He has stated on direct examination that he did find it.

The Master: Well, then, the question is answered.

Mr. Wiles: But I want to know where it is, in Pielsticker. He didn't give any line or page, but he simply stated that it was in Pielsticker, and I want him to tell me where it is. Where a patent is long, it is an immense saving of work if the witness will take it up—and, normally, in his direct examination, the witness takes and picks out the important things in it, and he has not chosen to do that in this case, but, rather, to make broad assertions, and I think we will have the record in such shape that, while it may be very long to read, it will be infinitely easier to handle in briefing, if we get these questions answered.

Mr. Rauber: Sometimes those features are shown throughout the context of the patent.

A. I fail to find any statement that it is either raised or lowered. We do have a heating scheme for it—just as we have in Ellis, though of a different type. In other words, it would agree with the Ellis specification.

Q. That is, it either might be raised, or it might not be raised?

A. Yes, sir.

Q. And you cannot tell, from the patent?

A. You could operate it in either way, of course.

Q. And there is no statement in the patent as to which way he does operate it?

A. There is no detailed statement in the patent as to which his preference is, just as there isn't in the case of Ellis, in the specifications.

Q. Now, what product is it that you understand Pielsticker was trying to make? That is, in the process described down to line 130, on page 2—and having in mind, of course, the date of 1890, when he was working?

A. I presume that he was trying to make kerosene of that date, as his primary product, and, incidentally, he would, of course, get certain other products, such as gasoline and gas and carbon, and so on.

Q. Now, I find, beginning at line 130, of this patent, "If the manufacture of permanent gas, or very light oils, is intended," that he then resorts to another process, which is a vapor phase, hot tube process. Do you understand that [fol. 540] to mean that if he wanted any such product as gasoline he would have used the other process—that is, the alternate process, beginning at line 130, page 2?

A. I would understand that he was making gasoline and kerosene with the original process, and that this concerned here with page 2, is similar to what Ellis discloses in his original specification, was—that is, a vapor phase process—and that, in Pielsticker's original one, here, it is largely a liquid phase process.

Q. Yes, but I am trying to get at the purpose—when he says if he wants very light oils, does he mean by that, gasoline?

A. I would think that it would be what we would call the light ends of gasoline—as other vapor phase processes make a quite light oil.

Q. Then, light ends, made in that way, are not gasoline?

A. Yes, sir, they may be considered as gasoline, but they are particularly important as putting in when a blended gasoline is made—say, by the Burton process—it needs a light gasoline to go with it—such as a casinghead gasoline.

Q. Your understanding of the Burton product is that it requires enrichment in its light ends—is that correct?

A. My understanding is that if you add light ends to it you can take off more products from the Burton process than you could if you did not add your light ends. In other words, that you will increase your yields more than corresponds to the light ends that you have used.

Q. Now, I want to get just your position on that.

A. On which—the Pielsticker, or Ellis, or Burton?

Q. No, on this Burton matter, that you have brought in here.

The Master: You brought Burton in?

Mr. Wiles: No, he used it as an illustration, your Honor—he said, "just as in the Burton process."

Q. What is your understanding of the percentages of light and heavy fractions, in the normal product of the Burton process—that is, has it got an over-plus of light, or an over-plus of heavy?

A. It has a shortage of light—if you take the run as a whole.

Q. That is, if you cut the Burton product to proper end points, to meet specifications, it will be short on the light fractions?

[fol. 541] A. I don't know this from personal knowledge, but I know that that was a statement that was made some years ago.

Q. Who made it?

A. I think that this was the occasion, because of which your Company—the Standard Oil Company of Indiana,—was interested, at one time, in, possibly, taking over the Greenstreet process, in order to add to and supplement the Burton product.

Q. Where did you learn that we were interested in taking over the Greenstreet process—that the Indiana Company was?

A. I don't remember just where it was.

The Master: Was it common rumor?

The Witness: Yes, your Honor.

Q. And it is on that common rumor that you base the belief that you expressed here, as an expert, that the Burton product, if cut to proper end points for gasoline, is lean in its light ends?

A. Or was, at that time.

Q. Or was, at that time?

A. That is my impression. As I said, I don't know it for a fact. I didn't make an analysis of it at that time.

Q. Do you know enough about the product, really, to have an opinion, now, as to whether it is lean in its light ends, or heavy ends, or where it is lean, if anywhere?

A. My impression is that it is lean in its light ends, and that you use in it, by preference, a casinghead gasoline, in blending. If you used casinghead gasoline in blending, then it is obvious, that it is lean in its light ends.

Q. And is that a common characteristic of like pressure distillation products—that they tend to be lean in their light ends—the modern gasoline?

A. What process do you refer to as like pressure processes?

Q. You have said that the several that you have talked about are just like the Burton process—is it the general tendency of those to be lean in their light ends?

A. I think that is probably more true of Burton, than of the Dubbs, for example, but I think, in general, that we are finding it desirable, with liquid phase cracked gasoline, to use them as blended products, preferably using some casinghead gasoline, or similar light material.

[fol. 542] Q. The blending is in order to enrich the light ends—that is what I am trying to get at. I don't question that all of this stuff is blended, but I want to be sure that you mean that it is blended to enrich the light ends.

A. When it is blended with casinghead gasoline I think there is no question but what that is the purpose.

Q. And, of course, condensing under pressure very greatly helps in the direction of enriching the light ends?

A. If there is sufficient of the light ends there.

Q. It would be much worse, would it not, if you did not condense under pressure?

A. The much would depend on how much was present. It would be not so good.

Q. The gain is in these very light ends that you say are already deficient, or are still deficient, in spite of the pressure condensing?

A. Yes.

The Master: You mean the light ends are the more profitable?

Mr. Wiles: No, I mean the pressure condensation increases the light ends—the proportion of the light ends—and that is the very point where it is deficient, according to the witness' testimony.

Q. Now, in considering this Ellis cracking process, Patent No. 1,415,232—and, perhaps, other patents—you drew, in your direct testimony, a sharp distinction between vapor phase and liquid phase cracking, did you not?

A. I drew a distinction, and I should say that it was fairly sharp, yes.

The Master: And by sharp you mean that it was plain?

The Witness: Yes, sir, that you would have no trouble in distinguishing between which ones were which, normally.

Q. Now, I want to develop that whole matter in much greater detail than you went into it in your direct testimony. I want to know just what these distinctions are, as fully as you can state them, with reference to products, the nature of the chemical changes involved, the difference in mechanism, or what happens, clear through, just as fully as you can tell us. And I think, perhaps, I had better let you give your full theory of it.

The Master: It might be well to start with a definition of what is meant by a liquid and a vapor phase.

A. By a liquid phase cracking we mean a process in which [fol. 543] the molecules which are undergoing decomposition, are primarily within the mass which is in liquid form. There is, in such a case, probably some cracking of the vapors which are above the liquid. This is relatively minor in amount, however.

In the vapor-phase cracking, on the other hand, we are concerned, here—where most of the cracking of the hydrocarbon molecules, takes place among those which are in the form of vapors, and not liquids.

We would then say, in a liquid phase the major portion of the oil passes through the cracking apparatus, in the form of liquid. Whereas, in the vapor phase, the portion which is being cracked, and which, normally, is the major portion, passes through in the form of a gas, or better, a vapor.

Mr. Rauber: That is not quite clear to me. Do you mean the vapor phase might start as a liquid, but it is heated after you get into the vapor phase?

The Witness: It is heated after you get it into the form of vapor.

Mr. Rauber: Do you mean part of it, or all, you get into the form of vapor?

The Witness: The part which is in the form of vapor, decomposing by the heat which is applied to it, cracks, and we would speak of that as a vapor phase cracking.

In vapor phase cracking, in most cases we heat to a higher temperature than we do in the case of liquid phase cracking. That is necessary, inasmuch as we want the materials to be in a vapor form, for that particular pressure.

The extremes, perhaps, would be such a patent as that of Palmer, patent No. 1,187,380, in which it is heated under conditions in which there is little or relatively little vapor present—and practically all is a liquid.

The extreme case in the other direction, might be, for example, such as the Ellis application was, as it was at the start, before the successive amendments—in which the oil vapors were heated and the products obtained are essentially in the form of vapor, plus a little carbon, and only a relatively small volume of residual liquid, carrying this carbon obtained.

Liquid phase processes were originally run at atmospheric pressure. They were later, in many cases, run under super-atmospheric pressure of considerable amount.

The vapor phase processes have been run—as in the case [fol. 544] of the original Ellis disclosure, as he says, on page 7, line 8, of the original specification—"preferably at slight pressure." But others, such as that of Rittman, run at higher pressures—super-atmospheric pressures.

The heat and fuel requirements are, of course, greater, in working a vapor phase cracking process, than in working a liquid phase cracking process.

The vapor phase cracking processes have the additional trouble that any scheme for the removal of carbon which might remain in the vapor phase cracking apparatus, is likely to give trouble, because of radiant heat—that is, the chain, or scraper used, absorbs heat radiated from the walls of the retort, and, as such, it, itself, soon gets carboned—that is, coke is deposited upon it.

In the vapor phase process, unless the speed of the vapors is quite high, we will have difficulty in keeping the carbon deposited, from adhering to the sides of the pipe, or retort.

In the case of the liquid phase this gives less trouble,

because the liquid will carry away, suspended in it, a certain amount of the carbon.

Do you really mean, by that question, to have me state as fully as I can? If you do, we are in for another hour.

Q. I really want it rather fully, Doctor. I don't want any unnecessary material in here. Of course, I want to know the chemical mechanism, and I want to direct your attention, particularly, to the fact that frequently these processes run, or can be run at the same temperature—for example, Ellis runs at a minimum temperature of 752, and, of course, we know that some liquid phase processes run over that. I want you to give your conditions in a parallel way, and tell me just what the distinctions are, chemically or molecularly, and why.

[fol. 545] Going, now, to the temperature relations which have been mentioned: Ellis gives 752 degrees Fahrenheit, —or, rather, its Centigrade equivalent—as his minimum temperature, and 1,112 degrees Fahrenheit as the maximum. I am quoting now, not from the specification, but from the claims.

Palmer, on the other hand, which I gave as a more nearly straight liquid phase cracking process, uses a temperature as his minimum, of only 393 degrees Fahrenheit—some more than 350 degrees lower.

The chemical mechanism of the two processes, has much in common. The physical carrying out of the processes, however, is quite different.

Considering, now, the products formed, we find that while they are, in general, carrying the same constituents, yet, in the case of the liquid phase cracking we normally have a less proportion of carbon and of permanent gases formed, than in the vapor phase cracking.

So that, as to the products we can say that the extremely light products are more likely to be formed by vapor phase cracking—including considerable amounts of permanent gases.

When making gasoline by the two processes, the vapor phase process would, in general, be expected to give somewhat larger percentages, not only of permanent gases, and of carbon, but also of olefinic materials—that is, unsaturated materials—and of the lighter fractions of gasoline.

The liquid phase process would, on the other hand, nor-

mally give more of the kerosene fractions, and less carbon; more of petroleum residues, which, however, might be redistilled, say by atmospheric pressure, and then recracked.

In each case, however, it is possible to operate the process so that it will vary from this general and normal procedure.

One of the biggest difference between the vapor phase cracking and liquid phase cracking, is in the question of time of exposure of the hydrocarbon, to the heat. In the vapor phase cracking it is normally a very short time exposure. In the liquid phase cracking, however, it may be [fol. 546] a considerable time—even running into hours. Vapor phase cracking, ordinarily, is less than a single minute.

Are there any phases of those that you would care to have me go into, further?

Q. I want to put one question, to bring out some details. You have, in your answer, given the conditions of operation, which you say are general—that is, they generally apply to this type of process. Now, it is perfectly possible to run a liquid phase process at, we will say, 760, isn't it?

A. Fahrenheit?

Q. Yes.

A. Yes, sir.

Q. And, also, to run a vapor phase process at 760—and I call your attention, particularly, to the fact that Ellis has 752 for his minimum.

A. My understanding is that this 752 was put in by him when he was later considering liquid phase work.

Q. Well, it is possible, isn't it?

A. I don't believe so, because here you would have a gas oil, for example, whose upper boiling point is distinctly above 750. , Or, if you were working with a crude oil, the boiling point—you would not have it as a vapor—unless you were working your vapor, possibly, at a vacuum.

Q. But I want to get a condition which can be paralleled. You can find a stock which will all vaporize below 750, without any difficulty, can you not?

A. Yes, you could use kerosene and crack it to get gasoline.

Q. And the light half of gas oil?

A. Yes.

Q. Now, if you take that for your cracking stock, you could run either liquid, or vapor phases, at 760?

A. Yes, if you run up to 800 you can run either one of them with normal gas oil, approximately?

A. Yes, approximately.

Q. Now, I want to get, and I am particularly interested in getting the differences which arise, not from differences in temperature, or differences in time, but those differences which arise because of the difference in phase. Now, suppose we take a liquid phase process and a vapor phase process, and have no difference, other than the difference in [fol. 547] phase, and the difference in pressure which is required to produce that phase—then, I want to know just what the mechanism of the difference between the two processes, is—the nature of the difference between them.

The Master: He means physiochemical reaction, I think.

Mr. Wiles: And the result, as well. That all washes out in the process, and——

The Master: You may proceed.

Q. Now, assuming that we have only the difference in phase and pressure necessary to produce it, I am trying to get down to what difference the phase makes.

The Master: I understand that both phases result in the decomposition of the original molecular structure of the crude oil.

Mr. Wiles: Of course.

The Master: Resolving them into atoms of hydrogen and hydrocarbons. Now, you want to know, after the process of decomposition has been reached, what are the differences in the combinations of atoms that result in the one, as distinguished from the other?

Mr. Wiles: Yes, your Honor, and the kind of products. He understands that I mean the chemical mechanism. I want everything reduced, as closely as possible, to——

The Master: I think I asked him the question, here, if any human being could know what took place inside of those closed chambers, under high pressure, and his answer was that nobody knew what took place, except by analyzing the product that came out.

Mr. Wiles: Yes, and this goes to that. Is there a difference in the product? If so, why? When the difference is only in the phase and pressures necessary to produce that difference, with stock of the same kind.

The Master: Doctor, we have fired away at you to give you a comprehension of something that Mr. Wiles is wanting to know, and I would be glad to know; if you can give an answer that will meet our desire.

A. As you have so much less material present per cubic foot we would expect the decompositions to take place to a greater extent, because we are furnishing, as I understand it, the same heat supply, and everything else excepting——

Q. Well, the temperature is the same, of course. You will not feed it so fast, because you have not got so much there, but the temperature is the same.

[fol. 548] A. Oh, in your vapor phase cracking, you will have particularly local overheating at the walls, compared with what you would in the case of liquid phase cracking, but the most obvious thing would be the output. One case would have about 200 times the output of the other, the vapor phase being only a small amount, the liquid phase a much larger, possibly 150 times. The figure 200 is only a rough figure.

The vapor phase, of course, is, because of its small capacity, subject to a great deal more in the way of variations in the operation. I would say that chemically it would be a question of the local and possibly irregular superheating in spots on the wall of the containing vessel, which would not occur so readily in the case of liquid phase.

Q. Let me suggest this: That for a pound of material, the same oil is held for the same time at the same temperature, the result will be the same—substantially the same, whether it is liquid phase or vapor phase, is that your position?

A. No, I think we would have somewhat different. We would have,—because of your local heating in the case of your vessel, and it would depend somewhat on the shape of the vessel. If the vessel was, say, of the same shape, one case a small drug so big (indicating), heated; and the other case a large drum so big, heated, each holding a pound, one of liquid and the other of gas, that in the heating of the big drum you would have local spots; that is, the bottom would be hotter and the top would not be so hot.

Q. That can be obviated by a lead bath or an oil bath, can it not?

A. No, it would not, Mr. Wiles: You will have, even there, your transfer heat. You have got some phases of

heat entering in, and that at the surface, that is, at the edge of the drum, will be hotter than it is inside.

Q. What I am trying to get at——

The Master: Given a certain process, and another certain process, how can one tell that one is a vapor process and the other is a liquid phase process? What determines it? How can you say which is which?

The Witness: You have in the vapor phase process a tempetrature high enough—by measuring your temperature and your pressure you could easily determine if you are above the boiling point of your hydrocarbons, and if you were, you would have the hydrocarbons in the vapor phase and you would be heating the vapors. Actually, [fol. 549] there is no difficulty to one who is at all familiar, in looking at a piece of apparatus operating and telling which it is.

The Master: I want to know——

The Witness: I would say that you can perhaps get here some evidence——

The Master: How would you classify this Cross process that I saw this morning, where the oil is pumped through the coil, heating the coil and re-pump through it, circulated through it until it reaches 800 degrees Fahrenheit under a pressure of about 700; then it is allowed to run into a re-action chamber where it is held during a certain period, and then the material from that blown out into an expansion chamber, where it expands to many times its volume, and from there conducted into a bubble tower; is it vapor, or is it liquid phase?

The Witness: That is liquid phase, because cracking occurs while it is in the tubes, and while it is in the digestion chamber, and when it expands there, the cracking is essentially through and they are removing the vapors.

The Master: When it expands in the re-action chamber or in the expansion chamber?

A. Expansion chamber; the cracking has been done.

The Master: There it is no longer under the high pressure.

The Witness: I say, at that time the cracking is essentially through, and as such only while it was in the liquid phase, was it cracking to any considerable extent.

Q. I am not sure that I got any definite answer to the question. What I am trying to get at, and I am trying to deal with this in a practical way, Doctor——

A. I understood you wanted it in a theoretic way.

Q. I am glad to know the mechanism that makes the practical difference, but I am really most interested in is whether the difference in phase makes a practical difference in the nature of the product; the mere differnce in phase, not differences in local heating; whether the mere difference in phase——

The Master: And not the differences in the ratio of the products, either; whether you get the same kind of products.

Q. Whether you get the same kind of products, then I want to know about the proportions, because that is of the greatest commercial importance. Are they essentially the same or essentially different?

A. The products are essentially the same; their proportions present will not be the same.

[fol. 550] Q. Will they differ to a commercially substantial extent?

Mr. Rauber: It seems to me there is a misunderstanding here. Are you speaking of high temperature vapor phase process?

Mr. Wiles: I am talking about—I want a comparison between a vapor phase and a liquid phase process, where the sole difference is the difference in phase and the pressure necessary to maintain the liquid phase when that is used.

Mr. Rauber: Assuming there might be such a process?

Mr. Wiles: Oh, it is conceded by the witness there can be, and it is obvious physically that there can be, Mr. Rauber; because you can take light gas oil and crack it either——

The Master: Let us assume it is. I am striving to be helpful in arriving at some understanding with the witness. I assume that this Ellis process with the electrical heater——

Mr. Wiles: Externally fired coil.

The Master: What?

Mr. Wiles: It is a fired coil.

Mr. Rauber: That is Adams.

Mr. Wiles: Ellis is a fired coil.

The Master: I meant Adams.

Mr. Wiles: But we are talking about Ellis.

The Master: Practically a flash.

Mr. Wiles: Adams.

The Master: Yes.

Mr. Wiles: That, I suppose, will depend upon the pressure but that was not the patent I was on now.

The Master: Take the two and compare them, for instance, the Cross and the Adams. There, one is liquid phase and the other is a vapor phase, is it not?

The Witness: Yes, as I recall the Adams. The Adams with electrical heater——

The Master: I want to know how to distinguish which is horse and which is cow.

Mr. Wiles: Now, I do not know that I have had an answer to my last question, Dr. McKee?

Q. Where there is only the difference in phase between the two processes and the pressure necessary to maintain the liquid phase. Of course, you have said that the products would be of the same kind but different in proportion. Now, do you understand that they would be different in proportion to a commercially noticeable, practical amount?

A. Yes, one would be roughly 200 times as much as the other, for apparatus of the same——

[fol. 551] Q. I do not mean in output, I meant the proportion of products with reference to each other, per pound of stock.

A. By products you mean constituents?

Q. Yes, the constitutents of the resultant products, would they be the same for practical and commercial purposes, or different?

A. I would say that they would be different.

Q. To what extent?

A. To a moderate extent. The vapor phase cracking, for example, will tend to give more carbon. It will, in general, give less residue liquid. It will probably give slightly more olefinic materials. Of course, I am not able to judge from the particular case that you mentioned where a cracking process is run on the two phases, using only the light half of the gas oil.

Q. I only picked that to give you the same conditions of stock.

A. Yes. I think it would apply even in that case. I am not saying that it would apply in all cases, though.

Q. You have referred in your testimony at times to the rate of conversion; would the rate of conversion be the same at the same temperature in the two phases or different?

A. I do not know.

Q. Now, the Master has spoken of the Cross process which he visited this morning in which the temperature is 850, in a process which you have said is essentially liquid phase. Now, that could be run and to do that the pressure is about 600 or upwards. That process could easily be run on the same stock in the vapor phase with a lower pressure?

A. What stock was it run on?

Q. Gas oil.

A. What is its range of boiling point?

Q. Well, 850 will carry it over, will it not, at atmospheric pressure?

A. Largely.

The Master: It was done at enormously high pressure.

Mr. Wiles: Yes.

Q. But for the same apparatus running at atmospheric pressure, it would be all gas phase, would it not, with the same stock?

A. Essentially so, if it was at 850 Fahrenheit.

Q. And your judgment is that if so run, would be substantially and noticeably different in the proportion of the [fol. 552] products, I mean in the constitution or the relation of the constituents of the finished products to each other?

A. Or given the number of pounds pressure, I think you would get, for example, more carbon and probably more permanent gas.

Q. How would the gasoline compare in percentage based on the stock and in its quality?

A. I would expect slightly more olefinic, if given the same time exposure.

Q. And about as much yield on the stock as you would get?

A. How is that?

Q. About the same amount of gasoline per gallon of stock used, or more or less?

A. I feel on these questions that it is hardly a fair thing when I have not seen the process and have not seen the

layout for it, to ask me quite that kind of a question. I can only answer in connection, essentially, with the disclosure in the patent.

The Master: Of course, you are only expected to answer what you expect theoretically from the laboratory side.

The Witness: Yes.

The Master: As long as you have not actually operated it. That is the way you are answering the questions, is it not?

The Witness: It is.

Q. One of the points made in this Ellis product patent, which is No. 1,318,060, is that this product is free from gumming or resenifying olefines. There is, is there not, a standard gum test laid down by the Government in its methods for testing motor fuels?

A. I presume you are referring to the residue left on evaporation, say, in a copper dish. I do not recall whether that is a Government specification or not.

Q. Well, the gum test is described in the Bureau of Mines Bulletins for testing motor fuels, is it not?

A. I think so; I am not certain.

[fol. 553] Q. Just to avoid any possible question, I call your attention to the Lefax abstract of Bureau of Mines Technical Paper No. 214, which describes the gum test on page 4.

A. That is the copper cup test which I have referred to in my answer to the previous question.

Q. And that provides for a maximum of gum, on the gum test, of .03 grams or 30 milligrams?

A. It is .03 per cent, as I recall it.

Q. Well, it is 100 cubic centimeters?

A. Yes, if you start with 100 cubic centimeters it would be .03 grams.

Q. And the Government specification for gasoline includes that requirement, does it not?

A. According to the Lefax abstract which you showed me, which is not itself a Government publication, of course.

Q. Now, how long has the gum test been known in the art; I mean as a method of testing motor fuels? How long has anybody appreciated the need of a gum test?

A. I think it has been appreciated—the need of it has been appreciated since gasoline was first made; gasoline, for example, which is used for cleaning purposes, so-called

naphtha for cleaning purposes would need to be free from gum, even more so than gasoline that is used for motor spirit.

The Master: In the dying industry would it make a difference?

The Witness: Yes, where the cloth was washed afterwards in gasoline; but I am referring particularly to cleaners who take our suits and give them a dry cleaning, or more particularly, the ladies' apparel which is of light color.

Q. Well, in those days, in the early days of gasoline, what was there that would gum?

A. There was very little; if the gasoline was carelessly made and some other products splashed over, it would, of course, gum.

Q. Ordinarily straight run gasoline will not run more than five or ten milligrams on the gum test, would it; ever?

[fol. 554] A. A shale oil gasoline, carelessly made——

Q. I said a straight run gasoline.

A. No, it would not. You asked me the question of what would.

Q. A straight run gasoline would?

A. A straight run gasoline, properly made, would not, normally.

Q. And nobody would sell shale oil gasoline for cleaners' handling?

A. I understand that they did and that it was the common gasoline for Scotland and England for that use in the periods of say 1880.

Q. Now, did they have a gum test there?

A. They undoubtedly would not want a gasoline carrying any gummy material when used for that purpose.

Q. Can you not answer my question?

A. (continued). Or whether they used the copper cup test, or any other gum test, I do not know. I do not know the history of the methods of testing for gum.

Q. Do you know, as a matter of fact, that there ever was any gum tests at all until recent years?

A. I recall fairly early statements on the question of putting light oil, such as gasoline or naphtha, on to white blotting paper and seeing if it would leave a colored residue. That would be, essentially, a gum test. That was before the

days of the present cracking processes to supply motor spirit for the present day automobile.

Q. If it was done on blotting paper, that involved no heat?

A. How was that?

Q. Being done on blotting paper, that did not involve any heat?

A. No, not to amount to anything.

Q. Just let it soak up?

A. And evaporate.

Q. Now, how important is this gum test which is made nowadays, as to motor fuels?

A. Moderately important. It depends upon the carburetors, with the better grade of carburetors, it is much less important; or rather, put it this way, with some carburetors, it is much less important than it would be with others. It is a question of clogging the orifice. If the orifice is a fine orifice it might clog easily.

Q. You can take gasoline that has a very high low gum [fol. 555] test, and it will not make very much trouble; that is a fact, is it not?

A. It depends, as I said, somewhat on the carburetor that is used.

Q. I mean, if it goes out on the market, a high-gum gasoline will not make trouble very often, and it will not make it very bad, either, will it?

A. I think they get away with a good deal in the way of gasoline on the market in this respect.

Q. Do you know that there is on the market a gasoline made by one of the leading refiners in this country that runs about 500 milligrams gum test?

A. It is new to me, so far as a particular figure is concerned.

Q. But if such a gasoline were in use, it would get by most of the time, would it not?

A. Oh, yes.

Q. But it might make trouble once in awhile.

A. If used in a particular type of carburetor and for a continued time.

Q. And when it did make trouble, it would not be an extremely annoying trouble to the driver?

The Master: Any trouble is extremely annoying to the driver.

Q. Well, this kind of trouble would not even let him limp home; he would have to take his carburetor apart and clean it on the road, would he not?

A. I do not know enough about carburetors to tell you or answer you that.

Q. Now, as I get your testimony on this Ellis product patent, you cited (and I want you to check with me) the Thorpe and Young reference——

A. I have not the copy of my testimony, so I cannot say.

Q. Have you not memoranda on which ones you used?

A. Only in part.

(Document handed by Mr. Rauber to the witness.)

Q. Now, these are the references that I have abstracted from that evidence: Thorpe and Young, Engler, Williams, Riebeck, Walker, Bassett and Boynton, and three references to Allen's book. Now, will you check and see if that is all that you had against that patent.

When I said "Williams," I meant Williams' publication in the Chemical News, by the way, Doctor. Mr. Dearborn [fol. 556] has called my attention, Doctor, to the fact that I omitted that note by Cabot in the Chemical News. That will make another one.

A. Did you mean to include Armstrong and Miller, page 358?

Q. Well, that is one question I want you to answer. You cited some on the treating and I was not sure whether you were citing them against the product or not.

A. Yes.

Q. You did not, in terms, cite them against the product?

A. I would say that——

Q. You did not mention them in the question that had to do with the product?

A. (continued). If we add Armstrong and Miller, I should say that includes those that I gave on the direct.

Q. Armstrong and Miller?

A. Yes.

Q. Now, referring to the Cabot first, your statement:

"That at one stage in the process the material would be in part uncracked, and partly cracked, and if condensed, would give a motor spirit."

A. What page?

Q. Page 356. Now, just what do you mean by that, please?

A. This is only a note, as I called attention, a brief note.

Q. Has it really anything to do with the Ellis patent or not? If it has not, we can pass it. I do not see that it has.

A. His process that he describes there would give a product which would correspond to the specification of Ellis, inasmuch as he has cracked his petroleum, and he is using a petroleum benzene, which, at that time was very cheap; that is, a gasoline costing around seven cents a gallon, that being the market price. And he calls attention that by cracking that you would get some benzol, and if we got that we would be getting an unsaturated product and we would have a part of our raw material left; and, accordingly, the product obtained would be similar to that of Ellis.

Q. Would it run a car?

A. I expect so. We can use and we are using at the present day, mixtures of naphtha, or this type of gasoline with benzol.

Q. Now, in the first place, this Cabot note was not any process that anybody ever worked, was it?

[fol. 557] A. No, it was a suggestion.

Q. A mere suggestion?

A. It was never a commercial process; it was simply a disclosure of a process.

Q. It never made any product at all; there never was any product made that way, ever?

A. I do not know whether anybody, after reading this, followed it out; they might have.

Q. The article itself is a mere suggestion in the form of a letter to the editor of the paper that possibly you might get some benzol if you put petroleum benzene through a hot tube; that is all there is in it, is there not?

A. I do not find there "all there is in it"; there is no such qualification. He simply suggests it as one which has, it is proved from the other chemical evidence, occurred. He does not say that he has carried it out.

Q. And he says, "It might lead to the obtention of benzol?"

A. He states that.

Q. And it does not indicate that any product ever was produced by this process in the history of the world?

A. Is that a statement or a question?

Q. That is a question. That is a fact, is it not?

Mr. Rauber: What is that question?

Mr. Wiles: There is nothing in the article to indicate that any product was ever made by this process in the history of the world.

Mr. Rauber: That is a statement.

The Master: It would have an effect as a publication.

Mr. Wiles: It would not, as a disclosure of the nature of the product, I think.

A. I think it would be well if I read the article here, the first part being only introductory. Well, I will read the whole thing:

"Sir: May I have space, as of old, in your hospitable publication for a discovery which I think is of considerable interest, both to scientific and industrial chemistry—conversion of the normal paraffins into the benzol series.

Here in the United States there are several processes of making illuminating gas from petroleum by passing it into heated retorts. I was informed by Mr. John Lerltus, of East Boston, that the tar from these gas works resembled [fol. 558] very much the tar from coal gas. I examined a specimen of this tar and found that it did contain the benzol compounds, but not either phenol or creosol. This points very distinctly to the decomposition of the normal paraffins of petroleum into benzol compounds on a rather large scale. It seems to me that it might lead to the obtention of benzol by passing petroleum benzine (which sells here at about seven cents per gallon) through tubes heated to the proper temperature in connection with the manufacture of gas the yield even if quite small, might be made to pay. At all events, the new methods of gas making may still afford us a source for benzol and its derivatives.

I am, sir,

S. Cabot, Jr."

This is a disclosure in the same sense as many a disclosure of a process.

Q. Does it mention a product?

A. Yes.

The Master: The product benzol.

Mr. Wiles: What?

The Master: The product benzol.

The Witness: The product containing benzol.

Q. Well, the product of Cabot's process is not Ellis' product, is it?

A. "In connection with the manufacture of gas, the yield even if quite small, might be made to pay."

It is not presumed and he does not state that his product is benzol. He says it is a product from which he can get benzol, and he suggests starting with a lighter material, gasoline, which at that time was called benzine, and cracking that and getting benzol. Now, his cracking would not be complete and the product then would be a mixture of benzol and gasoline.

Q. And what else?

A. Possibly carbon.

Q. And tar and pitch and all the other things that you get out of a hot tube process, is that not a fact?

A. Yes, in small amounts.

Q. Yes.

A. (continued). But the product, nevertheless, would be a product.

The Master: He means it would want separation?

The Witness: Simple distillation would give a product which would be of the same character as Ellis.

[fol. 559] Q. And in what proportions would the various ingredients be in it, if you can tell?

A. It would depend upon the degree and extent of the cracking. He could crack it so that he could get it or he would have some benzine left.

Q. Does the article tell what to do to it to get the Ellis product?

A. Not exactly, but if——

Q. I mean today, do you know what to do to it to get what he suggests, benzol, plus the Ellis product? Tell us how to make the Ellis product with a considerable yield of

benzol in it, in a single passing through a hot tube, using gasoline as your starting point?

A. On page 4 of Patent No. 1,318,060, lines 72 to 77:

"The herein described motor spirit, consisting——"

The Master: That is the Ellis patent?

The Witness: Yes.

"—consisting of gasoline-like material of a complex character, carrying a substantial amount of unsaturated bodies, and having greater volatility than gasoline of corresponding specific gravity—"

[fol. 560] If he thoroughly condensed his product from there——

Q. Which claim is that, Doctor?

A. No. 1. (Continued.) You would have such a product, because the benzol is unsaturated.

The Master: You mean probably a product as Cabot referred to?

The Witness: Yes, that is a benzol-cracked gasoline; that is a gasoline that has come over and has cracked and would be still lighter than it was when it started, if it was thoroughly condensed.

Q. And Cabot's raw product would not run a motor?

A. How is that?

Q. Cabot's raw product, just as he got it out of here, would not run a motor, a modern motor?

A. I think it would. Modern motors will stand a good deal of mistreatment, but I am referring to the question of the raw material, and for that material that is gotten out by taking the product and then simply distilling it with nothing more.

Q. Would the product meet modern motor fuel specifications?

A. I do not find any modern motor fuel specification in the Ellis patent.

Q. Well, the Ellis patent claims, does it not——

A. Say Claim No. 1.

Q. (continued). —in each claim a motor spirit, or substantially that language all the way through; that is the fact, is it not?

A. Yes.

Q. Now, the Ellis product, which is made by passing through a hot tube, requires treatment before it meets Ellis's gum test, does it not? I mean before it is freed from gum?

A. I think that is true but I cannot find it in the specification.

Q. On page 3 I think you will find it, Doctor.

A. I have gone through the patent specification rapidly and I do not find it even on page 3, where you suggest.

Q. Are you looking at Ellis Patent No. 1,318,060?

A. Yes.

Q. Ellis does treat to get rid of gum, does he not?

A. He does not so state. He does treat with acid, but he does not state what the purpose is, so far I have been able to find.

[fol. 561] Q. Well, what is the purpose?

A. How is that?

Q. What is the purpose stated?

A. To get a water white product, in several places, is mentioned.

Q. Now, the ordinary hot tube cracked produce will run high in gum, will it not?

A. I would expect a vapor phase cracking process, such as Ellis' hot tube, to be fairly high in gum.

Q. And the Cabot product would be way up in gum, of course?

A. About equally so.

Q. So that's Cabot's—the most you could contend for Cabot is that his product, as he gets it out of his hot tube, would be somewhere near like Ellis's product before treatment, differing by containing considerable quantities of benzol?

A. The distillation of the product away from the carbon and residue would largely remove the gum of Cabot.

The Master: And of Ellis?

Mr. Wiles: And of Ellis?

The Witness: Ellis does not still afterwards.

The Master: Oh.

Q. Now, if distillation removes the gum, Doctor, why does everybody treat to meet gum tests in this modern day?

A. They treat also for other purposes, not gum alone.

Mr. Rauber: Mr. Wiles, reading this Claim 1 of Ellis, I find it does not say anything about the gum. Are you asking questions in regard to that claim?

Mr. Wiles: I am asking him about Ellis's preferred product. I think it perfectly well settled that no matter how broad a man's claim is, it cannot cover more than the equivalents of the actual things he describes, and it should not ever cover less. The effort of the Court being to find out what the particular thing described in the patent is, and construe to cover that thing, the comparison being always between two physical materials.

The Witness: Ellis calls attention that on evaporating in an open dish here he gets a greasy material, resembling very soft vaseline, which would prabably be the gummy part, but he removes it with acid instead of by evaporation. That is, by distillation, he could get it out unquestionably, in part.

[fol. 562] Q. He gets rid of his gum by acid treatment; I mean gum forming constituents, of course?

A. Yes, but he does more than that by his acid treatment.

Q. Yes, of course; but one of the things that he has to do to get rid of his gum forming constituents is to treat with acid?

A. Yes.

Q. That is one of the things he does do, does he not?

A. It is one of the things he does do, yes.

Q. And that treatment gets rid of the gum forming constituents?

A. To a considerable extent.

Q. And Cabot does not do that?

A. It is obvious that you would distill off—if you had carbon and other materials, you would remove by the ordinary scheme of distillation. It is not my thought that Cabot uses the same process of getting rid of his gummy materials, but that that would be a natural one, well known in the art at that time, and an obvious one.

Q. And your position is that simple distillation will get rid of the gumming constituents?

A. In part.

Q. Gum forming constituents?

A. In part. Sufficiently to have met the description that Ellis gives of his product.

Q. In all of his claims where he specifies "free from resinifying products"?

A. No, but at least in——

Q. I want his specific product, Doctor. Do not let us quibble about the words of a claim. I want his specific product, which he says is "gum free". Now, would distillation, in your judgment, get a gum free product or not? It is a perfectly simple question.

Mr. Rauber: You mean could you by distillation get rid of the gummy products, or do you always; which do you mean?

Mr. Wiles: Would the simple distillation he is talking about get rid of the gum free product, and if it would I would like to know why all my clients use so much acid.

The Witness: Mr. Wiles, listen to your question as it is read. I do not think it will read as you intend.

(Question repeated by the reporter.)

Mr. Wiles: I do not intend to quibble about words because

[Omission in copy.—Printer.]

[fol. 563] Q. I mean simple distillation, would that free a product from those constituents which form gum?

A. Yes, to a considerable exent.

Q. Enough to meet gum tests?

A. If the amount present is not too large it would be sufficient to permit its use in the modern motor car; in an average motor car.

Q. Now, do not dodge my question, please. We have taken an awful lot of time. I asked you whether you could meet gum tests with it, not whether it would run in a car. I know there is gasoline that runs a car up to 500 milligrams, even if you do not know it. I want to know whether it will meet gum tests which gasoline today ought to meet, and which Ellis says his product does meet.

A. Was there a gum test in 1913 when his application was filed? I think not.

Q. Yes, but he says it is substantially free.

A. Oh, those are the things that he mentioned after he had made his invention.

Q. I am talking about his patent. I will argue the file wrapper with the Court and not with you.

Now, just tell me this: Will a simple distillation (this is a fact, not a mat-er of argument about the Ellis patent) as a matter of fact, will a simple distillation of a hot tube cracked product, like Cabot's render the distillate substantially free from those bodies which form gum?

Mr. Myers: The only difference is you changed from "gum tests" to "substantially free".

The Master: Answer that question; answer it either yes or no, if you can.

A. It would depend, to some extent, on the raw material used, whether gasoline——

Q. I am talking about Cabot's material.

A. Using gasoline as Cabot did *wor* his raw material, or rather benzene, I know that it would remove a considerable amount, but I am uncertain whether it would remove sufficient to meet the 1925 copper vessel scheme of testing.

Q. Would it be "substantially free"?

[fol. 564] A. I am not in a position to interpret the claim on that, and that does not occur, as I recall, in any place in the specification.

Q. It appears on page 4:

"My product in its preferred aspects has an iodine number of not less than 30, but preferably higher and even maybe in excess of 75 or 100, under some circumstances, and in its preferred form is substantially free from olefines, readily forming resineous bodies on exposure to air."

A. I did not recall that that was in the specifications. I noticed it in the claims but not in the specification.

The Master: Assuming it to be, what is "substantially free"? How free has it got to be, to be "substantially free"?

The Witness: I would say that was free enough to permit it to be used in a motor car engine, and I would say that Cabot's product, after distillation, would be that free. It would not, however, probably come to the .03 per cent

residue, which is the present—Navy, is it, or aviation?

Mr. Wiles: Navy.

The Witness: Gasoline.

Q. Now, as a matter of fact, Ellis gasoline does meet, if you follow the patent—Ellis' gasoline does actually meet modern gum tests, does it not?

A. I do not know.

Q. And the defendants' cracked gasoline meets modern gum tests, even more than half, do they not?

A. How is that?

Q. The defendants' cracked gasoline, at least my client's gasoline——

The Master: Which defendant?

Mr. Wiles: My client, the Indiana Company's cracked gasoline, will have a fifty per cent margin under modern gum tests, will it not?

The Witness: I have never tested your Company's gasoline.

The Master: In order to identify it, I ask you if your product is made by the Ellis process.

Mr. Wiles: It is distilled under the Burton process, as we contend, and treated by the Ellis process.

The Master: That makes the Ellis patent pertinent to you?

Mr. Wiles: Extremely.

The Master: Of course, as a licensee you are interested in sustaining the patent.

Mr. Wiles: Certainly.

Q. What do you find in the Cabot, any better than any hot tube cracking process, as a reference to the Ellis product?

[fol. 565] A. He happens to mention the products formed, which is not true in many of the hot tube cracking processes, as many of them describe their process and speak of making gas, but they have not analyzed the liquid products, and as such it makes a better reference. There are undoubtedly many plants which have made a product which completely filled the requirements of the Ellis product, but which, it happens to be, we do not have, or at least we have not found written descriptions of their product.

Q. You know that all of these really hot tube processes,

the gas-making processes, make some benzol, as a by-product?

A. In general.

Q. So then, why is it——

A. But that was not known, as I understand it, at the time of the Cabot publication.

Q. It was known in Ellis' time, was it not?

A. Oh, yes, Ellis knew it.

Q. At Ellis' date, Cabot did not add anything to all the common, well known hot tube gas making processes, every one of which made a little benzol as a by-product.

A. But Cabot's date was 1877. Of course, Cabot did not add anything to Ellis'.

Q. And Cabot's raw material is not any better for this purpose than the tar thrown down from any common oil gas cracking process, is it?

A. Yes, because he starts here—he proposes to start with a benzine, which would at once give us a second material besides the benzol. It would make a product meeting the Ellis specifications.

Q. Did you ever see any of these real hot tube processes where any of the raw material got through in its original condition? Is it not all cracked up and re-synthesized and changed around?

A. They are largely when you are taking a heavy hydrocarbon, a large hydrocarbon molecule; using light hydrocarbon molecules, however, such as he suggests, the benzine—one would expect a considerable portion to get through, particularly if you did not carry your cracking to the extreme limit, but carried it to a normal amount.

Q. But if you want the maximum of benzol, you have got to carry it to the extreme limit, have you not?

A. If you want the maximum yield.

Q. In other words, Cabot's notion was to make benzol, was it not?

[fol. 566] A. No.

The Master: That was the object of his article, was to make benzol?

The Witness: Not quite, your Honor. "In connection with the manufacture of gas, the yield, even if quite small——"

Q. He suggests it as a scheme for getting more benzol, does he not?

A. "In connection with the manufacture of gas"—yes, not as——

Q. Gas and benzol is what he is after?

A. Yes, the two.

Q. In other words, to get gas and benzol, you are going to crack to the very limit possible, are you not; just as far as you can go?

A. No, if you will heat your benzol up you can get gas out of it, and you can crack benzol by heating further.

Q. But if you want nothing but gas and benzol, you will go just as far as you can go in the first passing, will you not?

A. No.

Q. And they do, actually?

A. (continued). Because if you went as far as you could go, you would have no benzol left.

Q. Get all gas?

A. You would decompose practically all your benzol that you would have formed in the first part of your tube; that would have decomposed to give carbon and hydrogen, or carbon and smaller hydrocarbons, such as ethylene.

Q. You would get all gas and little carbon?

A. You could carry it through, yes.

Q. Do you not know, Doctor, that if you could do what you have just stated, you would be surpassing the best efforts of the entire gas making industry?

A. It can be done, has been done, but it cannot be done economically; that is the question. If you will take a hot tube and heat it very hot you can complete the cracking clear down until you have practically only carbon and practically no benzol.

[fol. 567] Q. That is not an economical process, you say?

A. Oh, it is not an economical process; I have not pretended it is.

Q. Just where was that ever done?

A. It has b*ene* done, for example, in university laboratories.

Q. Where?

A. I thin*g* Berthelot did some work of that kind in France, and Davidson, working under Zeneth, did some work on cracking down lower than benzol.

Q. And they got nothing but gas and carbon?

A. How is that?

Q. They got nothing but gas and carbon?

A. I think the largest molecule that they got was butylene with a $C_4 H_8$ molecule. They can carry it by using more moderate temperatures and get some benzol.

Here is a commercial process that does it, I had forgotten about this. In Louisiana, they are cracking a hydrocarbon and getting carbon and hydrogen and essentially no by-products. I am referring to the scheme worked out by Dr. Brownlee for making carbon black. They do have some methane uncracked afterwards. The product they are starting with is a hydrocarbon which is not gasoline or oil; it is a gaseous hydrocarbon.

Q. They start with a gas and they subject it to combustion with insufficient air, and it smokes like a lamp, and you get all gas left and some soot; that is about the process, is it not?

A. No, I think you are thinking of another process, Mr. Wiles. The scheme that I am thinking of gives you hydrogen as a by-product, which would not be true of such a process as you have described.

[fol. 568] The Witness: It is a question as to whether a man in 1911 would use the same clay retorts as was probably used thirty-five years earlier in the retorts mentioned by Cabot. However, presuming that he would use the ordinary type of gas retorts made of clay, with a D shaped cross section, the flat side of the D being the bottom of the retort, and heated them externally with coal or other fuel to a high temperature. The method of laying out the retorts would be essentially that customary in gas plants; that is, several retorts high, two or possibly three or even four. In such a case as this I would think better results would be obtained by using only two retorts, one above the other. These retorts would be joined to inlet and outlet pipes, the outlet pipe being one joined to a suction blower, so that there would be a slight, but only a slight vacuum in the retort, say of one-half inch to one and a half inches of water pressure, or in that general range.

The inlet oil might be flowed in directly or it might be sprayed into these hot retorts.

The outlet gases would be passed through some sort of a cooling device, and either completely cooled and vapors

precipitated out of them, or cooled to approximately ordinary temperature and then sent into an absorption tower, where they would be treated with a wash oil, such as a rather high boiling mineral oil having a low vapor pressure, this being the customary type now used and then used (that in 1911 used) in gas plants for the removal of benzol and similar vapors from illuminating gas made from coal.

This wash oil through which the gasses would pass counter-current would be drawn off and reworked for the vapors which it has absorbed from the gas. The gas would be passed on to a storage tank, that is a gas holder, provided they were pure enough; that is, free enough from sulphur and similar products; if not, they would have to go through a purification house such as the ordinary gas plant contains.

[fol. 569] The benzolated oil or wash oil containing the benzol and other vapors which it has absorbed, would be sent to a still and steam distilled. The steam distillation would remove the benzol and benzine (gas oline), and this product would then be condensed, probably sent to another building by means of a pipe-line, and redistilled and cut at the proper point. It might require, and possibly would require, a chemical treatment, in order to be made into products of the highest character.

The Master: You mean into commercial benzol?

The Witness: Into commercial benzol or a benzine mixture; commercial benzol and gasoline mixture.

I did not state, but I should have stated that the retorts would be fed according to Cabot with benzine (gasoline) and if the retorts were at the right temperature and the feed was proper, we would get through a considerable portion of this benzine or gasoline, unchanged, and this would be absorbed by the wash oil and come out with the benzol.

So that we would have as a final product a motor spirit, high in unsaturates, due to the benzol constituents, and also to any olefines which would be produced by the cracking.

It is quite probable that when such a plant started up the yield of good motor spirit would not be as large as after they had been operating for sometime and found out the optinum temperatures to which to heat and the maximum amount that could be fed to produce the desired gas and the desired condensate.

I have not attempted in answering this to describe all the phases, as to how the furnace would be built and similar phases of the unessential items.

Q. Now, still on this same issue of directness of this teaching, I notice that the Cabot article, while it refers to the gas making industry, as using retorts, suggests tubes for his scheme. Do you draw a distinction there or why did you shift from what he suggested to the retorts?

A. Cabot says, "Here in the United States there are several methods of making illuminating gas from petroleum, by passing it into heated retorts——"

In other words, Cabot says, "retorts."

Q. Yes, but go on down the paragraph when he talks about his own suggestion, he says, "tubes."

A. "Tubes." I would say that in my suggestion here tubes could be used as the equivalent of retorts.

Q. It would make no difference?

[fol. 570] A. Not any essential difference, excepting that in the case here you would not want to have your tubes too small or carbon formation in the cracking process would tend to clog them.

Q. Now, how hot would you heat these, bearing in mind that what Cabot wanted was the manufacture of gas and benzol?

The Master: If I understand the publication, it was a suggestion for making benzol.

Mr. Wiles: What?

The Master: If I understand the publication, it was the suggestion of making benzol. Cabot is leaving out of sight gas, that is illuminating gas. He has in mind the production of benzol.

Mr. Wiles: I went with the witness that far. He insisted yesterday that he contemplated both and I was willing to go with him on that, although I quite agree with your Honor, that benzol is what he really had in mind.

Mr. Rauber: You mean that you know of any case where they get benzol or cracked gasoline without getting any gas?

Mr. Wiles: Oh, no. Of course, he gets some gas.

The Master: That is quite beside the mark. Here is a man that says, "I know how to make benzol"; "here is the way to make benzol."

Mr. Wiles: Put it that way, Doctor: "Here is the way to make benzol," what would your temperature be?

The Witness: I am inclined to think, quoting here again: "In connection with the manufacture of gas, the yield (meaning the yield of benzol), even if quite small might be made to pay."

The Master: That is, a plant producing benzol alone might not be commercially profitable, but if united with an illuminating gas plant, you might make the two successfully, that is all there is to that.

Mr. Wiles: Yes, sir.

The Master: They are doing that every day with the by-products in the gas plants; toluol and benzol are being made and turned out as part of their products and being sold as part of their products of their operation.

Mr. Wiles: Yes.

A. As I recall the temperature used in the manufacture of benzol, that is the distillation of coal, it is approximately a thousand degrees.

Q. Centigrade?

[fol. 571] A. Yes.

The Master: What is that Fahrenheit?

Q. 1832?

A. 1832 Fahrenheit.

Q. That is the temperature that you think one skilled in the art would run Cabot at?

Mr. Rauber: Temperature of the furnace you mean?

Mr. Wiles: The retort temperature.

The Witness: That was the temperature at which gas plants normally operated when they are operating at the high temperature to make present day illuminating gas; when they are operating to make a larger yield of hydrocarbons they operate at a decidedly lower temperature.

Q. Benzol, not generally hydrocarbon, but benzol.

The Master: Just keep your mind fixed on the production of benzol from benzine and forget the gas operation entirely. That is Cabot's disclosure. What temperature would be required in the cracking tube or retort in order to operate and produce benzol by operating on benzine.

The Witness: I would say a temperature such as Niki-

foroff suggests for the same operation of about 750 degrees Centigrade, or 800 degrees Centigrade.

The Master: That is how much Fahrenheit?

The Witness: That is 1,382 Fahrenheit.

Q. How much?

A. 1,382.

Q. Now, how long would you leave the product in that zone; I mean the material in the zone of that temperature for the manufacture of benzol? That is, what is the time element?

A. Oh, it would only be a part of a minute.

Q. Well, I want the part of a minute because I will do this thing just the way you tell me.

A. It is impossible to say without just knowing what the feed it. I have suggested the rate of feed which would develop there.

Q. The feed determines the time element, does it not?

A. Yes, that is what I say.

Q. Now, how much time element would one skilled in the art do to make benzol without experiment?

[fol. 572] A. If we held it there for a half a minute, I would expect to get benzol. Whether that would be the optimum yield or optimum production of the plant is very seriously questioned by myself.

Q. But your recommendation would be to start this thing off at 1,382 degrees Fahrenheit and an average rate which would produce a time in that zone of thirty seconds?

A. I think we would be safe in so doing and that we would get benzol.

Q. I did not ask you what would be safe; I asked whether that is the best one skilled in the art could do as a start.

The Master: You mean whether it is best or not, whether by using that temperature and time one could get benzol?

A. I feel that we would get benzol.

Q. And is that the time period for treatment which you think one skilled in the art would use, if attempting, in the light of the Cabot disclosure, to practice the process he suggested?

A. It is the temperature and time which I would suggest as probaly being one satisfactory—that I would suggest to a plant operator to start off the plant operation upon. I do not think, however, that that is anything more than a guess as to that.

The Master: Let me ask: Knowing the difficulties of handling extremely high temperatures, would one skilled in the art use lower temperature and longer time to accomplish the result?

The Witness: Yes, Actually, if I was serving as manager of that plant, I would not attempt to make benzol the first try. The first thing I would do would be to send through gas slightly above atmospheric pressure without any heat to see if the apparatus leaks; the second think I would do would be to heat there to see if leaks developed on heating. Then, after we had found that the apparatus was [fol. 573] leak proof, essentially so, we would try the question of running in the "benzine" (gasoline) of Cabot, and would gradually raise the temperature to see what the results obtained were and keep on raising until we got to the point where we got the benzol. That was not the question that was asked me, though, as to the method of starting up the plant.

Q. In your outline of the way you would build and operate this plant given in the beginning this afternoon, you said you would first cool down—in your preferred method you would first cool down to a normal temperature and then scrub with wash water; was I right in that? Is that a correct understanding?

A. Yes, to approximately normal temperature.

Q. By that you mean what, 100 degrees, something like that?

A. 100 Fahrenheit.

Q. And in that cooling you would get some material precipitated, would you not?

A. There would be carbon dust carried over by the process; probably there would be a little dust from the retorts, and there might be some tar precipitated.

Q. Some tar? Now, that tar——

A. I mean by that, heavy tar.

Q. Now, that tar would not be the motor fuel you are talking about?

The Master: Would there be some liquid developed?

The Witness: Well, that tar would be hot and as such it would be liquid.

Q. That would not be the motor fuel that you are saying would be naturally and inevitably produced in the operation of the Cabot process?

A. No.

Q. Now, that would be a heavy thick thing like coal tar when it was cooled?

A. Yes. I am not certain that starting with a raw material like benzine, we would get an appreciable amount, but possibly we would.

Q. Whatever it was that came out in getting down to 100, would not be the motor fuel, anyhow; 100 Fahrenheit?

A. No.

Q. You would scrub your gas of 100 with an oil like mineral seal, or something like that?

A. Yes.

[fol. 574] Q. And the first thing you would get then would be a heavy mixture, principally composed of mineral seal?

The Master: Answer yes or no.

A. Yes, you would get a mineral seal solution of your motor spirits.

Q. And in that the mineral seal would be far the greater proportion and the other ingredients would be very minor proportion?

A. Yes, minor; I do not know that we should say "very minor."

Q. At least seventy-five per cent mineral seal?

A. Yes.

Q. And 25 per cent of the other? I will modify my question: 90 to 95 per cent mineral seal and 5 per cent up to possibly 10 per cent of the other ingredients?

A. It might be as much as 25 per cent of the other ingredients. It would depend on your temperature to cool; about how clear you wanted your gas from it.

Q. Now, that mixture would not be motor fuel?

A. No.

Q. Now, you take that over and you run that mixture in a still with steam taking off an overhead, do you not?

A. Taking off what?

Q. Taking off an overhead distillate from that mineral seal mixture?

A. Yes, you would probably run it into a column still.

Q. And you would put a cut point on it somewhere, would you not? That is, how would you make your cuts when you make your distillate?

A. In a column still you would draw off the particular portions.

Q. Yes. Now, when you want benzol, as you are running this plant—you are running this plant for benzol?

A. For benzol, yes.

Q. Where would your last cut be made?

A. The last part that comes off through the dephlegamator into your condenser.

Q. That would be your benzol and lighter gas?

A. Benzol and gasoline.

Q. Benzol and lighter gas?

A. Yes.

Q. And the endpoint on that cut in a proper bubble tower would be where, for benzol manufacture?

A. If we only wanted benzol itself——

[fol. 575] Q. That is what you are after in the Cabot process. What would that cut be; where would your endpoint be on the overhead?

A. If we wanted only benzol, our cut would be at about 200 degrees Fahrenheit.

Q. And of that product that came over at 200 what proportion would be benzol?

A. Feeding it as suggested with gasoline, probably one-third.

Q. When you are running for the maximum of benzol you could not get but one-third of the fractions below 200 benzol?

A. Using benzine as Cabot suggests as a raw material, we might readily not get more than one-third.

Q. Now, you would have, you say, one cut with an endpoint of 200; what other cuts would you take, just a heavy bottom cut which you would re-run for benzol; as you suggested yesterday?

A. I do not know just what you mean by "heavy bottom cut."

Q. I beg your pardon, I was wrong on that question, Doctor, because you are distilling out of mineral seal and you would want to have your mineral seal, of course. Now, you would take another cut between mineral seal and benzol, which would be all the rest between 200 and the end; that is right, is it not?

[fol. 576] A. That is right, yes, and that would be re-run probably in the process.

Q. That you would re-run in the process. Now, it is this first cut with an endpoint of 200 that you say is motor spirit, am I right in that?

A. Yes.

The Master: Now, Doctor, assuming that one wished to perform this operation. Would he look at the prior art containing such things as Dewar and Redwood for a suggestion as to the building of his furnace and his piping and his necessary concomitants of his structure to perform his operation?

A. Yes, but not particularly Dewar and Redwood. I think we would find much better.

The Master: As for instance?

The Witness: Well, in Intschick, the Russian, and in a number of the ones which are concerned with higher temperature crackings, such as Dewar and Redwood.

There occurs to me one phrase I used that possibly should be explained a little. I said "cut at approximately 200." I said also that we would be speaking of a steam distillation. I really meant cut where we would if we were dry distilling at 200, but we were carrying it out with a steam distillation.

Q. What the question was, and I think you answered it correctly, was that for benzol you would run til the endpoint of your overhead or distillate was 200?

A. If that was the question I answered it correctly; if it was in the other point, simply as to where the cut would be, then it needs the explanation that I have added.

Q. That overhead distillate has an endpoint of 200?

A. Yes.

Q. We are together on that?

A. Yes.

Q. Now, I want to take up the Thorp & Young reference.

The Master: Before you leave that, I want to know what "unsaturated bodies of the aliphatic series, corresponding to an iodine number of about 75" means. I do not know what that means at all; it is Greek to me.

The Witness: That would mean, in general, the products that we have been speaking of as olefines, or it would also include our diolefines; that is, ones that have two unsaturated groups in them. If we used the term "unsaturated bodies" without specifying "of the aliphatic series" we

would then include as well such products as benzol and several others.

[fol. 577] The Master: But if you use "unsaturated bodies of the aliphatic series" you would not include benzol?

The Witness: That is right.

Q. And as shown by you the other day, the benzol has a very low iodine number?

A. Yes; however, a relatively high sulphuric acid unsaturation.

The Master: Of course, if you had a spirit containing a substantial amount of unstable, unsaturated bodies, then you might include benzol?

The Witness: It would never be unstable; it might be stable, it might be stable and still include it; for example here the first claim.

The Master: But if those requirements were there, that it should be "stable, saturated bodies——"

The Witness: Benzol would still fit.

Q. Benzol is extremely stable, is it not?

A. Exceptionally.

Q. And these unsaturated aliphatic compounds are, some of them, of the unstable character?

A. Particularly diolefines.

Q. Exactly. And are the olefines more or less stable than the benzol?

A. The olefines are less stable than the benzol towards most, though not all, chemical re-agents.

Q. How about the things that happen in normal handling of fuels, such for example, exposure to light, and exposure for long times to elevated temperatures, as in the tropics, and so forth?

A. I would expect that the olefine hydrocarbons would be less stable than benzol.

Q. Much more or very little different?

A. I would expect those with larger molecules to be decidedly less stable.

Q. It is the heavy ones that are likely to go off?

A. What does "off" mean?

Q. I mean which would break down and give you the undesirable results that instability gives you.

[fol. 578] A. Heavy ones are particularly susceptible to chemical action.

Q. Yes. And they are particularly susceptible to these other re-actions, like light and long continued temperature and the normal oxidation and the other things that happen to fuel when it has to be stored and handled?

A. I am not very certain in answer to the last question. I have seen statements to that effect; I have also seen statements that it was not that; people thought they had olefines of large molecular weight when they had some olefines in it, which were particularly sensitive.

Q. These troubles, I suppose we both understand, that occur to fuel, happen by reason of something in the higher part—higher boiling part of the fuel or the lower boiling part of the fuel?

A. Both, but possibly more in the upper half of the fuel.

Q. Considerably more?

A. I do not know. We do know this; that larger molecules ordinarily have more color to them and if we increase the size of the molecules, having even the same chemical reaction; we are likely to increase the amount of color disproportionately as compared with what we increase the size of the molecule, so I cannot say as to that phase with certainty.

Q. Now, I want to take up this Thorpe and Young publication.

A. Which one? There are several.

Q. Well, the group which you used as a reference against this Ellis patent, and if I am not mistaken, the part that you used as a reference was the description of the product which they got by distilling in their iron mercury bottles; that is correct is it not?

A. In the first disclosure in the earliest article, they describe it as a product made on the larger scales. In their second article they say the same thing can be carried out in pieces of strong composition tubing, that is, in glass.

In the third article, they describe both methods, I think, so that we have two descriptions of the use of glass apparatus and two of the use of larger quantities.

Q. Those are several papers by the same people, before different bodies or different times before the same body, covering about the same work, aren't they?

A. Yes.

Q. And they were really two experiments—one was in [fol. 579] glass, and the other was a larger scale experiment in mercury bottles—is that true?

A. I think that is correct.

Q. And the one you rely on here, is the detailed description of the mercury bottle result, isn't it?

A. Yes, but he states that the two are essentially the same.

Q. Now, in this operation with the mercury bottles, they had about a gallon, in all, of their total product—that is right, isn't it?

A. A little over a gallon.

The Master: What is a mercury bottle?

The Witness: A mercury bottle is—we generally speak of it in this country as a flask. It is a bottle in which mercury is shipped. It is really an iron vessel, made, essentially, bottle shaped. Glass wouldn't stand it—and carrying, I think, 75 pounds of mercury—I am not quite certain as to the exact commercial amount, but it is about 75 pounds. So that that would make it about a gallon and a half container, made of iron, and made so that it can be closed up—that is, threaded at the top.

The Master: What Thorpe and Young were describing was a laboratory experiment, and the kind of an instrument that they used in this laboratory experiment, when they said that "we used a mercury bottle?"

The Witness: That is all—it is simply an ordinary piece of apparatus that would be around a laboratory as they buy mercury in the laboratory—it would come to them in this form, and they used two of those containers.

Mr. Wiles: And they made a little still, using two of those cast iron bottles that the mercury comes in. One they used for a still, and the other, just like it, they used for a condenser and dipped it in water.

Q. Is it your contention that this gallon of product that they first made—their first product—is a motor spirit, like the Ellis motor spirit?

A. No, I would say that the product that they described as a distillate, when they spoke of distilling into two or three fractions.

Q. Well, all right, I will come to that. We would save lots of time if you would just answer the question and let me put the next one.

Now, having this original material, they cut that into three cuts—the first cut was that which boiled below a hun-

dred Centigrade, or 212 Fahrenheit, amounting to three-tenths of a litre—that is correct, isn't it?

[fol. 580] A. So I understand.

Q. How much is that three-tenths of a litre, in gallons?

The Master: Ask him a fair question and we will have done with it. This was a laboratory experiment, made with such apparatus as one would pick up around a laboratory and perform a laboratory experiment?

The Witness: Yes, that is correct,—but a large sized laboratory experiment.

The Master: Well, the usual size for an ordinary laboratory experiment?

The Witness: Yes, sir.

The Master: Why waste further time on it?

A. Answering your question—about a twelfth of a gallon.

Q. Now, is that twelfth of a gallon the motor spirit that you set up here?

A. What do you mean by "set up here?"

Q. Is that first twelfth of a gallon boiling below 212 Fahrenheit, the motor spirit which you say is the Ellis motor spirit?

A. It could be used as a motor spirit, and it would be such as is described by Ellis, but, as I recall it, I said that we would mix the first two fractions——

Q. Thorpe and Young didn't do that, did they? They separated them?

A. They were after getting out the individual constituents. This was only a step in getting the individual constituents.

Q. Exactly. Maybe I am wrong in my theory, but——

The Master: Now, let us get this. Was the result of the Thorpe and Young laboratory experiment a motor spirit consisting of gasoline-like material, of a complex character, containing a substantial amount of unsaturated bodies?

The Witness. Absolutely.

The Master: And having greater volatility than gasoline of corresponding specific gravity?

The Witness: It was.

Mr. Wiles: Just a moment. You- Honor's question assumes that there was a single product in the Thorpe and [fol. 581] Young process. I am trying not to waste time,

but maybe I can save a lot of this discussion, back and forth, if I state my position.

The Master: Go to it, without any further discussion with me. Go to it with the witness.

Q. Now, the point I am trying to get at is this: Thorpe and Young ended up with a very large number of separate products, and I want to know which—that is what I am inquiring about—where, in this process, did they have a motor spirit? And I am not going to dispute with you about the possibility of blending up, out of anticedent, or other materials, an Ellis motor spirit. If you went to the chemist's shelves and picked out the stuff—you could blend them, if somebody told you how?

The Master: You mean, could they, at the final end of their process, fractionate off a substance which would correspond to that claim?

Mr. Wiles: Did they, at any time, do it? Did they have anything, here, that was motor spirit?

A. I could answer that very quickly by saying that at that time there were no motors, but in 1911, a man, reading this, could, without difficulty, by making his endpoint 200 degrees C, have a product which would be a motor spirit in every sense similar to that of Ellis.

The Master: By following the process followed by Thorpe and Young for its reduction, and using apparatus known in the prior art, he could have arrived at that motor spirit?

The Witness: Yes, sir.

Mr. Wiles: If somebody told him how.

The Master: No, no—he says that is in the disclosure. He says he would draw it off at the proper fractionating point.

Mr. Wiles: If somebody told him how to do it. I want to get these questions of mine answered, if I can.

Q. Now, this twelfth of a gallon of stuff that boiled below 212 Fahrenheit, which was Thorpe and Young's first cut,—now, is it your position that that cut—which was the actual thing they had—corresponds to the Ellis motor spirit?

A. It corresponds to Ellis' description of his motor spirit. It could be used in a modern car as a motor spirit

It would be too good a product to be used as a motor spirit commercially, because we could use just as well a poorer product, and thereby make it cheaper.

Q. Now, that particular product, boiling up to 212, is one of the Thorpe and Young fractions, which you say is the Ellis motor spirit, is that right?

[fol. 582] A. I say it corresponds to his description of his motor spirit.

The Master: When you say "his," you mean Ellis?

The Witness: Yes, sir.

The Master: Do you think that if you have made your claim too broad, you can save your claim, now, by a disclaimer?

Mr. Wiles: You can alway disclaim, provided there are other claims. But there are some very specific claims here, your Honor, and I don't think any of these are met, but I want the witness's position on these specific products. That is what I am after, and I am entitled to get a comparison on it. Some of these claims are extremely narrow—that is, they are specific, as to Ellis' product.

Q. Now, will you tell me how this first cut, boiling up to 212, about a twelfth of a gallon, Thorpe and Young's corresponds to the specific thing described in the Ellis patent, as his motor spirit?

A. It must be borne in mind that Thorpe and Young gave it a refining process, as well. Do you mean to cut out the refining process?

Q. Just a moment. I am just going that far—this first cut—at that stage, did that correspond to the Ellis motor spirit?

A. It corresponded to the Ellis description of his motor spirit.

Q. I mean, the physical thing that Ellis described?

The Master: Have you ever seen any of Ellis' physical thing?

The Witness: No, your Honor.

The Master: Well, then, you cannot answer.

Q. Well, it is fully described,

The Master: He has told you that it corresponds to his description of it.

Mr. Wiles: The broad claims, he is talking about.

The Master: Which of your claims do you want him to compare with?

Mr. Wiles: What I really wanted compared with is the specification.

Q. How does it compare, Doctor, with the physical material described in the Ellis specifications?

The Master: That is on which page?

Mr. Wiles: There is a long description of it.

The Master: There is a whole lot of process in this, and [fol. 583] it gets down, finally—I think it is at the bottom of page 8, about line 120.

The Witness: I would say that it would correspond to that of lines 117 to 125.

I would say, further, that it would not correspond to what he gives as a specific instance, having a Baume of 52 degrees.

It is a better gasoline than a 52 degree gasoline would be—that is, a higher grade product.

Q. And would it meet modern motor fuel specifications?

The Master: When you come to your theory of confining your claim, and saving your claim, by reference to your specification, you have got to deal with this very general description which might correspond to the first claim.

The Witness: It probably could be used, but I doubt if it would meet modern motor fuel specifications, which deal essentially, with the presence of olefines, until it has been carried through the operations given four line below the table on the next page—page 188,—"after repeated distillations over sodium." That is refining by the scheme of sodium.

Q. I will come to that in a minute. I will get to it all, before we get through, and we will save lots of time if you will answer the question. Do you think the first cut, boiling to a hundred, would not meet modern specifications for motor fuel oil?

A. These low boiling products, as was said earlier this afternoon, are generally relatively free from the color producing products, and, as such, it is quite possible—in fact, we would have to repeat the experiment to find out whether

it was true, or not—that this low boiling product, boiling below a hundred degrees, might be free enough from gum and color material, to meet the specifications for motor fuel—and there is an even chance that there would be. I cannot say definitely, because I have not carried it out.

The Master: What if you re-distill it?

The Witness: Oh, if you take it and re-distill it, it would.

Mr. Wiles: I just want to find out what Thorpe and Young did—not what they might have done.

The Witness: Well, they did it.

Q. What about the endpoint on this—do you think a product boiling with an endpoint of 212, would meet Navy specifications?

A. You are choosing the specifications which are the hardest to meet. The State specifications, for example——

The Master: This don't have to meet the Navy specifica-
[fol. 584] tions. When it is put through the process of Thorpe and Young, would it meet Ellis, and not the Navy specifications—that is the question.

Mr. Wiles: I have assumed, all the way through, that Ellis describes, and his claim is limited to a practical, usable motor spirit. It says so in every claim. I will withdraw the question.

Q. Do you think that this first cut, of which there was only one-twelfth of a gallon, would be a proper and suitable and safe fuel to put in a motor car and run it?

A. As I said once before, it was too volatile, too good a gasoline to be used commercially. It would be a better product, in the way of gasoline, than the ordinary user would want, because it would cost him more, in the first place, and, in the second place, it would be quite volatile. On a day like this it could be used without the slighest difficulty.

Q. And when you say "too good," do you mean it would be more satisfactory to the user than ordinary gasoline would be—forgetting the price, altogether?

A. It would be, as I said, rather volatile, and unnecessarily volatile, but it could be used. Whether it would be more satisfactory, or not, would depend upon the kind of carburetor he had in his engine.

Q. Now, take that first product, that was distilled over sodium, and cut into three separate cuts, wasn't it?

A. After repeated distillations over sodium.

Q. It was repeatedly distilled over sodium into those cuts, wasn't it? Isn't that your understanding? That is, that that was all the distillation and re-distillation necessary to get it into these very narrow cuts?

A. I am inclined to think that the text, reading as it does, would imply that the cuts were only made towards the latter part of it, though it might have possibly happened then, because he says "after repeated distillation over sodium, this portion was almost completely resolved into three fractions."

Q. But if you will look at the narrowness of those fractions—one of them 32 to 38, and 65 to 70, and the other 94 to 97——

A. There is no difficulty, with laboratory apparatus in making cuts as close as that——

Q. Wait a minute—with the large gap between them—doesn't that indicate that the repeated distillations were required to get them into those narrow cuts?

A. If we would use a Young column—a glass column about two and a half feet high—on top of our flask, such as is known as a Young column—it is probable that we could accomplish what they have accomplished in a single distillation.

If, on the other hand, they used no Young column, but used a simple flask, without any type of column, then it would take repeated distillations to do it.

Q. Doesn't the fact that they used the repeated distillations and got these very narrow cuts, indicate that that is the way they did it?

A. I don't know.

Q. It might be. I will withdraw the question. You don't know. Now, this first major fraction was put into these three little cuts, and of the first one, as I recall it, there was less than five cubic centimeters—is that right? That is what hangs in my head.

A. I think not.

Q. Yes—didn't amount to five cubic Centimeters—that is correct, isn't it?

A. No. That is the part boiling below the first cut. The [fol. 585] first cut was 32 to 38, and the part boiling 30 did not amount to five cubic centimeters.

Q. You are right. This first cut, then, 32 to 38—does he state how much that was?

A. I believe not.

Q. He said it was far too small to attempt the preparation of any derivatives from it, didn't he?

A. But he did prepare a derivative from it.

Q. No—the cut from 32 to 38—didn't he say that?

A. He prepared a bromide—treated it with bromine—and he also obtained quintane, which we, at the present day, call pentane.

Q. Then, does he state how much there was in this first little cut—can you tell?

A. No, he says that the pentane obtained was far too small to attempt the preparation of any derivatives from it. The brominated portion he analyzed, and identified, and——

Q. I understand. But the total of the first cut of the first main fraction, you can't tell how much there was of?

A. There is no statement here as to how much it was. The derivative he obtained, you notice he gives as amyline bromide.

Q. Is it your impression that that first smaller cut, or secondary cut, of 32 to 38, Centrigrade, is a suitable motor fuel, or motor spirit?

A. It is a material commonly present in gasoline, as, for example, in the Bulletin, No. 15, of the Kansas City Testing Laboratory, it says:

"Ordinary gasoline made by natural distillation of Mid-Continental crude, will contain several or all of the following substances."

The first substance it gives is pentane—the material that he got out here.

Q. Now, do you understand that that is what I asked you?

A. I thought I was answering your question.

Q. I asked you,—and I wish you would answer the question, and not something else—whether this material, boiling from 32 to 38 Centigrade, is, in itself a suitable motor fuel?

A. No, it is too low boiling, excepting for the first end of the material.

Q. Now, the second cut of the first major fraction was 65 to 70 Centigrade—was that cut a suitable motor fuel?

A. That could be used in winter time without any trouble, as a motor fuel for ordinary cars.

Q. It is as light as casinghead, isn't it?

[fol. 586] A. Yes, but it boils at about 150 Fahrenheit.

Q. And you say that is a suitable motor fuel?

A. I said it could be used in the winter time as a motor fuel.

Q. Satisfactorily?

Mr. Rauber: For what motors?

A. Satisfactorily in many cars, for out door work. It is too volatile to be used in the summer time, and possibly in some motors.

Mr. Rauber: Your Honor, I don't think these questions should be limited to motor cars. There is nothing said in the Ellis patent about motor cars. There are aeroplanes and other motors.

Q. Now, the third production that Thorpe and Young had from this first major cut, boiling at 94 to 97 Centigrade,—is that a suitable motor fuel—a satisfactory motor fuel?

A. It is a constituent of motor fuel, which meets your fifty per cent requirement of aviation gasoline.

Q. Now, do you think that answers the question?

A. Well, I don't think your question is at all a fair one. You are not at all fair in attempting to quibble, here, as to what a motor fuel is. You know and I know what it is, and why try to mislead in the matter?

Q. Just answer my question, whether you think it is fair, or not?

A. I don't think it is fair.

Q. The Master will stop me, if my questions are not fair. If you will just answer them we will get along a whole lot better. Would this cut, 94 to 97, in your judgment, be a satisfactory motor fuel?

A. Yes, when mixed with a small amount of lower boiling product.

Q. How about it by itself?

A. I don't know. It is about—wait a moment—I do know—yes, it will work in many types of motors, particu-

larly those which ordinarily operate on kerosene. This one would operate without difficultly, in certain motors. It lacks the low boiling point that would be required in some motors which depend upon having a low boiling constituent.

Q. The third cut, boiling above 200, you would not consider a suitable and proper motor fuel, I presume?

[fol. 587] A. It would be a suitable motor fuel for the type of tractors which use, in some cases, kerosene, or products intermediate between kerosene and gasoline. It would not be a satisfactory motor fuel for those motors which normally use low boiling products, such as gasoline.

Q. And that is true of any of the higher fractions that are above 200?

A. It is always true in general, of——

Q. All right. Now, you stated, I believe, that these first three little cuts, from the first fraction below a hundred, you couldn't tell as to the presence or absence of gum-forming ingredients, in the first place, because of their low boiling point—that is correct, is it not?

A. No, that is not correct.

Q. Didn't you say that you couldn't tell whether that fraction below a hundred, would have any gum-forming constituents in it?

A. I can, if you will give it to me and let me test it.

Q. I mean, from the publication.

Mr. Rauber: Do you mean if it was treated with sodium?

Mr. Wiles: No, before it came over. I thought I was stating his position.

The Witness: No, you have misquoted me, in part.

Q. What is your position?

A. I said that I didn't know whether that low boiling product would carry color and gum-forming constituents, but I was inclined to think that it probably would be low enough to be permitted to be used without any further treatment, because it was a low boiling constituent, but that I had not tested it.

Q. When we come to a fraction between a hundred and two hundred—the original, second main cut—that consisted of one litre—a quart, didn't it?

A. A litre is about a quart, yes.

Q. And that was the amount of that cut?

A. Yes.

Q. Would that carry these gum-forming ingredients?

A. It might, until it was distilled over sodium, as given on page 192.

Q. Do you think it would, as it came over?

A. Came over sodium treatment, or before?

Q. No, no—the first distillation, which was without sodium—would that cut into three major ingredients, from a hundred to two hundred, I am talking about—the first major cut, or about a quart.

[fol. 588] Now, in your opinion, did that cut contain these gum-forming constituents, or not?

A. Probably some, but whether enough to be bothersome, I don't know.

Q. Now, take that cut as a whole, a hundred to two hundred—is that a suitable motor fuel?

A. For some types of motors, yes.

Q. For the ordinary gas engine of 1912 and 1913.

A. They never use a liquid in a gas engine—or fuel—you use it in a gasoline engine, but not in a gas engine.

Q. Now, would the second primary cut, from the Thorpe and Young operation, consisting of about a quart, boiling from a hundred to two hundred—that is, 212 to 392 Fahrenheit—be, in your judgment, a suitable fuel for the running of an ordinary automobile engine, of the ordinary type, existing in Ellis' day.

A. I don't think it would. It would need about fifteen per cent more of lower boiling products—perhaps ten per cent would be ample—though it would work in other types of explosion motors, other than the one——

The Master: Internal combustion motors?

The Witness: Yes, other than the one that you particularly mention.

Q. Now, that second cut, consisting of about a quart, was subdivided by Thorpe and Young, into how many smaller fractions? Just tell me the number.

A. Into four fractions, which were then subdivided afterwards.

Q. Four of those were further subdivided.

A. Yes, sir.

[fol. 589] Q. How many did he get, finally, out of that four?

A. I think he obtained eight materials out of the four.

Q. He finally got eight lots?

A. The second sub-division was partially by chemical treatment and partly by distillation.

Q. There were four primary cuts?

A. Yes, sir.

Q. Now, would any of those products, or cuts, alone, by themselves, have been a suitable fuel for use in a motor car in Ellis' day? If so, which ones?

A. As we go up here, we, in general, narrow the type of motor spirit, and the narrower the group of internal combustion motors, that it can be used in. Limiting it to the narrow limits that you did, of motor cars—and I presumed you referred to internal combustion engines, rather than to motor cars of the steam type, such as the White——

Q. I didn't mean the Stanley steamer, or anything of that kind. I referred to internal combustion engines.

A. It would be an undesirable motor spirit.

The Master: That is, before it was refined by re-distilling?

The Witness: And cracking, and adding to it the light ends, such as casinghead.

Q. The point was not that before refining, it was not the right product—that is, this was the final product after refining, when he got done—no one of these cuts was a suitable motor fuel?

A. That is, of course, true, with ordinary gasoline—that no one of the narrow cuts is suitable motor fuel.

The Master: That is not Ellis' patent. Ellis' patent takes ordinary gasoline and refines it.

Q. Now, you have stated that this distillation over sodium would remove the gum which would originally be present after this major cut, No. 2, boiling from 212 to 392—what is the evidence for that statement, please? Gum-forming constituents, is what I mean.

A. It is a common laboratory method of removing such products. It is also a commercial—or, rather, described as a possible commercial method of causing chemical actions on diolefines, which are the products, which we understand, generally, as producing gum.

I refer particularly to the Perkin patent, on the use of sodium on diolefines, and to the work of the German Com-

pany, of which Duisberg was the managing director—I think it is the Elberfeld Company—in which they used it in a commercial way, though only in a small production.

[fol. 590] Q. When was it learned that the gum-forming constituents are the diolefines?

A. That has been the general impression for a considerable time. Certainly we know that the diolefines are much more re-active, chemically, than the monolefines.

In my earlier testimony I said that this was a common belief. I don't know the history of it, and there may be some error in it, but it is the common belief.

Q. What is the first mention in the literature, anywhere, of the presence of any gasoline, or a gasoline-like material, of these gum-forming ingredients?

A. I don't know what the first mention is.

Q. As far as you know, Ellis may have discovered them, and removed them both, for the first time, as a conscious action, may he not?

Mr. Rauber: Does Ellis mention diolefines?

Mr. Wiles: No, but I asked him where he found the first mention of gum-forming matter in gasoline, at all.

A. I know that it has been commonly considered that the diolefines are responsible for color and responsible for gummy materials.

I know, further, that sulphuric acid is a material which, when diolefines are treated with it, gives a tarry product, and as sulphuric acid treatment of the hydrocarbons, to give tarry products, have been known for many, many years—well, back in the kerosene art, of half a century ago—then one would think that this scheme of removal, and the inference that it was due to the products which gave tarry material, with sulphuric acid, was known, perhaps, at that time—fifty years ago.

Q. It is a fact, isn't it, Doctor, that when you treat an oil—many oils that do not have these gum-forming ingredients in them—with sulphuric acid, you will get a tarry product?

A. What is the proof that they do not have them?

Q. I did not ask that. You used that as an argument on the proposition that these gum-forming constituents were known. Now, I ask you if sulphuric acid treatments, with

all kinds of oils, whether they had gum forming constituents or not, does not give a tarry product?

A. No.

Q. Now, suppose we take an oil, say a cracked product, which has been treated so as to meet the gum test——

A. How is that?

Q. Suppose we take an oil, gasoline, a cracked gasoline, [fol. 591] which has been sufficiently treated to meet the gum test so that it is substantially free from gum-forming ingredients; you know there are such oils as that, do you not?

A. Yes.

Q. Now, you take an oil like that and treat it with sulphuric acid, 66 gravity, you will throw down a tarry product, will you not?

A. I would expect you to.

Q. Yes. In other words, the throwing down of a tarry product does not in any way indicate that the oil had in it any of these gum forming ingredients? Can you answer that question?

A. It certainly does. It is only with oil which has had special treatment, and which is an abnormal oil, so far as a straight distillation is concerned, that would have in it olefines or have in it such products which would give sulphuric acid tar, and at the same time not have gummy materials in it. It probably would have a small amount of gummy materials and it would not be free.

On the other hand, if you take an old type gasoline that has been treated with sulphuric acid, you can have a gasoline or you can have a gasoline which, on treatment with sulphuric acid does not throw down a tarry material and does not have in it the diolefines or olefines.

Q. Yes. But you can have a very light fraction of gasoline which will not have any appreciable gum forming constituents in it anyway, can you not? I mean, even a cracked one?

A. Yes, particularly if you do not have diolefines in it in any considerable amount.

Q. If you take a cut from the gasoline up to 225, it will not have any gum forming constituents in it left; not any you could notice?

A. Well, it would be low in it but I do not know as I should say it would be entirely free, that it would not have any.

Q. It would meet gum tests all right, would it not?

A. Probably.

Q. Yet, that same oil could be treated with sulphuric acid and throw down a very tarry residue.

A. I do not know possibly it may.

[fol. 592] Q. It would if it was fairly unsaturated, as a matter of course, would it not?

Mr. Rauber: You mean having gum forming constituents?

Mr. Wiles: No, no.

Mr. Rauber: Or if it did not have gum forming constituents?

Mr. Wiles: Just olefines.

A. It would depend on the temperature at which it was treated and it would depend on what olefines it had in it.

Q. I mean the ordinary treatment with enough concentrated sulphuric acid, Doctor.

Mr. Rauber: What are you treating?

Mr. Wiles: Light gasoline; light cracked gasoline.

A. That is a product boiling below 225 Fahrenheit?

Q. Yes.

A. Well, that gasoline—and I am taking your word that that is a gasoline——

Q. I said a cut from a gasoline.

A. No, you said, "that gasoline."

Q. I said a cut from a gasoline.

The Witness: Will you read the record, Mr. Reporter?

(Record read by the reporter.)

Mr. Wiles: If I did I misspoke myself. We will not quibble on words, Doctor.

A. You might have.

Q. So that the formation of a tarry residue after sulphuric acid treatment does not, of necessity, indicate the presence in the oil of gumming ingredients?

A. Not absolutely.

Q. All right.

A. But nine chances out of ten that it would be true.

Q. Now, who before Ellis ever discovered or mentioned in the world, these gum forming ingredients in gasoline, as distinguished from unsaturates such as the olefines?

Mr. Rauber: Are you speaking of the prior art? Of course, he has not made a search on that particular thing, and if he said, "no"——

Mr. Wiles: In the art that he has got.

Mr. Rauber: So far as he knows, you mean?

Mr. Wiles: Surely.

Mr. Rauber: If he finds one tomorrow he can bring it in?

Mr. Wiles: Surely, who, in the whole world, before Ellis; I do not know who has. Certainly I can not find it in the art he has got here.

A. I do not know whether they were interested in the question of gum forming constituents, but it is interesting [fol. 593] that they treated petroleum distillates by the same method that Ellis is using, many years before Ellis did it; and probably for the same purpose.

Q. That we will have to discuss later, but that does not answer my question.

Do you know of any mention in the literature of this art before Ellis of these gum forming ingredients in gasoline at all, as distinguished from ordinary olefines?

A. If we mean by the "gum forming materials" products which gave gum and color to the gasoline, we have in the early tests which I mentioned yesterday or day before, the equivalent of it; that is, putting the product on a piece of paper and allowing it to evaporate and seeing if a colored ring was left. That ring would be colored, and it would be, essentially, resinous material because if it was not resinous it would evaporate and the color would not remain.

Q. And it might be asphalt, too, that got in by some puking of the still.

A. Oh, yes, and it might have been yellow dye added.

Q. If you would just answer my question we would save a lot of time. Do you know of any mention in the literature at all of these constituents in gasoline which form gum on exposure to the air, as distinguished from ordinary olefines?

A. I can not at this time quote such a disclosure. I have not looked for such and can not now do it.

Q. And that being so, of course you would not know of any reference or teaching that you could remove the gum forming constitutents, meaning those which form gum from standing and exposure, without removing all the olefines or the bulk of them?

A. Yes.

Q. Where?

A. The old methods of treating illuminating oil with sulphuric acid in order to get a lighter product and a product which would not gum up the wick.

Q. Those are the same ones you have referred to in your testimony, are they?

A. No, I do not think I have referred to those; possibly I did. It was a process that was in use back in 1860, or possibly '65 at least.

Q. And the process was what?

A. The stirring up of the products with sulphuric acid. I have here citations for——

The Master: They have already been stated in the record, the process of the sulphuric acid treatment, have they not?

[fol. 594] Mr. Wiles: Some of them, yes.

The Master: There were a lot of them; talked about them.

The Witness: Yes.

The Master: Were there not citations of sulphuric acid treatments?

The Witness: Yes.

Q. You cited several special ones, if there are any others——

Mr. Wiles: If he is relying on something different from what he has cited, why, I want it; if he is not, I will go to what he has cited.

The Master: I do not know, but there were some somewhere that were read, an article on sulphuric acid treatment.

Mr. Wiles: Sure. If that is the one, why, we are together.

The Master: I know it recommended a light solution and not a heavy solution of sulphuric acid.

Mr. Wiles: Yes.

The Witness: Here, for example, I find in Mills' book on Destructive Distillation, published in London in 1892, page 30, speaking from shale oil distillates:

"They were formerly stirred with 2 per cent by volume of caustic soda solution in order to take up phenol and its analogues (kreasote) acetic bodies and perhaps some terpenes; the sodic extract was drawn off below and the

supernatant fluid, sometimes after washing with water, agitated with 5 per cent of oil of vitriol of specific gravity 1.7 (a metal stirrer or an air-current produces the required agitation). This latter liquid has but little action on the fatty hydrides proper; but on hydrides containing less hydrogen it acts powerfully, resinifying and polymerizing them as it does turpentine.

"Weak acid, sometimes used at this stage, is comparatively inefficient for the purpose."

I want to call attention that here he speaks of it as resinifying the product and as polymerizing these materials which are lower in hydrogen; that is, as we would speak of them as the unsaturates and also, further, that he uses an oil of vitriol. Oil of vitriol is defined, and it is interesting in this connection, because elsewhere we do not have it defined, as 1.7 specific gravity and not one of 1.84.

In other words, it is about a 77 per cent sulphuric acid and not a 93 per cent sulphuric acid. It would be 77,17 per cent. So that we have here in this early work a description of use of sulphuric acid for resinifying and polymerizing these products, using the same strength of sulphuric acid as Ellis asks.

Q. Now, Doctor, you missed my question or did not answer it, I don't know which.

[fol. 595] A. I think I answered it.

The Master: He did; he answered it. You asked him to point out where the prior art showed anything of that kind.

Mr. Wiles: Maybe I have lost the trend.

Q. Does that article that you have just read or quoted from show anything more than the removal of the olefines?

A. When we treat olefines with sulphuric acid, we ordinarily think of our sulphuric acid combining with it directly and giving olefines-sulphuric acid compounds. When we treat diolefines with sodium or with sulphuric acid, in many cases, we get polymerization, and here he mentions specifically polymerization.

Q. Your point is——

The Master: Resinous-like material?

The Witness: Yes, resinous-like material.

Q. Your point is that the olefines do not polymerize with sulphuric acid?

A. They do, but they do not polymerize near as easily as diolefines do.

The Master: The object of that treatment was for the purpose, not of destroying the olefines, but destroying the resinifying elements, was it not?

The Witness: That is correct.

Q. In that article that you read from just now, does it not show that the olefines were removed?

A. It says, "The action of hydrides containing less hydrogen" which would include olefines and diolefines.

Q. Certainly, and they are all——

A. (continued). But it does not state that it has removed them all.

Q. Is it not the assumption that they are removed?

A. Completely?

Q. Yes.

A. No.

Q. Is there any indication that they are not?

The Master: Does it not tell you how to avoid doing so, by the strength of your acid?

Mr. Wiles: Not as he read it.

A. The amount of acid used would be so small that it would remove all of it. 5 per cent would be too little, stirred as he says here, "with a metal stirrer or an air-current" in the few minutes.

Q. Now, Doctor, it is rather a recent discovery, is it not, that it is the diolefines that give these gums on standing?

A. No.

[fol. 596] I am reading from page 24, American Petroleum and Refining, and it is not a discovery of this author, but he is quoting from there——

Q. By whom?

A. (continued). "The diolefines hydrocarbons undoubtedly cause the petroleum refiner more annoyance than any of the other groups of hydrocarbons of which petroleum and its products are composed. Members of this series polymerize readily with a consequent darkening of the product containing them. Diolefines are produced in relatively large quantities by some of the present methods of cracking gas oil to produce gasoline."

Q. Who wrote that and when?

A. This is, as I say, not an attempt to describe late things.

Q. Will you please answer my question? Who wrote it and when?

A. Bell wrote it in 1923.

Q. Now——

A. (continued). But you will find it in almost any book. It happens to be that I had it here. In older books you can find it.

Q. Will you find it for me before Ellis, that is what I am after? I want to know who, before Ellis, knew that it was diolefines?

A. I do not recall that Ellis states that it is diolefines.

Q. No, he does not, but who knew it? You assume it. Did anybody know that before Ellis?

Mr. Rauber: Know just about as much before Ellis or afterward, is that it?

Mr. Wiles: Are you testifying?

Mr. Rauber: No, I am asking you if that is what you mean.

Mr. Wiles: He has relied on this recent knowledge. Now, who before Ellis' day knew that it was the diolefines that made you any trouble, or in fact there was any gum trouble at all?

A. The properties of diolefines were well known; that is, they are not well known now but were fairly well known as compared with the present day, even well before Ellis, even at the times of 1890 or such as the book I have just read.

Q. Citation, please?

A. Mills.

Q. Does Mills say anything about them? Please read it.

A. No, he does not; he is not interested and does not give you the formulae of the products.

The Master: Name an author that does.

[fol. 597] The Witness: Beilstein.

Q. And Beilstein tells you, does he, that those are found in gasoline?

A. I do not recall. I do not know that he does tell, but he gives the properties.

Q. Now, whoever knew before Ellis, is what I am trying to get at, that these things were in gasoline at all.

A. I do not recall any mention before or after Ellis, excepting the one citation I have read here, which is not a new citation. So far as an individual citation, naming the man, as to who did, I cannot, but it is common knowledge and I think it was well known even before Ellis, though I do not happen to recall the citations.:

Q. Now, you have said that this statement about the diolefines was not in the literature, as far as you know, also that you know of nothing in the literature telling us about there being any gum forming ingredients in gasoline, as distinguished from the other olefines?

A. I think that is not correct. I quoted you the blotting paper experiment, which were tests. They did not call them that but it was a test for that kind of a thing.

Q. You stated that would show other things besides, too, did you not?

A. I do not believe I did, excepting where you suggested that somebody might put in asphalt and I suggested putting in a yellow dye if they wanted to.

Q. I did not say they might put in a yellow dye or asphalt. You know, do you not, that stills puke sometimes?

A. Yes.

The Master: Stills what?

Mr. Wiles: Puke.

Q. And you know, too, that a still that is running along at the end of its run has some pretty dirty oil in its worm, do you not; may have dark oil in the worm?

A. It may have.

Q. It is bound to have, is it not, if it runs down to a heavy cut?

[fol. 598] A. The last distillate is likely to be a run of that character.

Q. Then, when you get your first gasoline off of your next run, it comes through a dirty worm and it may come through a worm that has just puked, where there has just been a puke through it, may it not?

A. It might, if it was so operated.

Q. What?

A. If the plant was so operated.

Q. Well, the plant often gets dirty that way in spite of the best attention, does it not?

A. I would not think so often. If the plant was operating under good management, I would not think so.

Q. Do you know of——

A. I would expect that not more than a small percentage of it.

Q. Exactly, but sometimes there is a puke so that your gasoline comes over off-color at the beginning.

A. The gasoline is washing out the material that is in the condenser.

Q. Yes, exactly. And then your blotting paper test would just test it for that, would it not?

A. It would test it for more than that.

Q. Exactly. It would test for anything but it is not essentially and necessarily a test for gum forming ingredients, is it?

A. No, but it is a test——

Q. And the fact that people made it does not indicate that they knew there were any gum forming ingredients there, because they might make it to test simply for this off-color which we have been discussing.

A. I do not think they were likely to make a test which would simply take an abnormal material. All they were after was to test to see if the product was carelessly manufactured, not from distillation, but from other things as well, such as the presence of gum forming materials.

Q. Did they not make that blotting paper test on straight run gasoline?

A. Primarily on straight run gasoline which was to be used as a cleaning material.

Q. Exactly. And that straight run gasoline would not have any gum forming material in it at all, would it; it naturally would not have?

A. Probably not, but they wanted to test for it to see if there was such material.

Q. The only sense in that test is to see whether the gaso- [fol. 599] line is off-color, as it could be from the puke; that is true, is it not?

A. No, it might be, and oftentimes was, off-color due to incomplete refining.

Q. Yes. And they were testing it for being off-color, and they would test it in the kind of gasoline that never dreamed of having any gum in it.

A. This would not be a test for off-color. They can see the color easier by looking through a glass vessel, but this would be a test to see if there was any non-volatile material of gummy character.

Q. Such as you would get in washing out a worm of light gasoline?

A. Such as you would get from light gasoline, and you might say gum.

Q. And you would get from a lot of other places? Heavy oils would give just that same reaction—you would get a ring on your blotting paper, would you not?

A. Yes, I would expect so.

Q. And the other things are very much more likely to occur in straight run gasoline than gum?

A. Possibly, but it was a test of gum, just the same.

Q. Yes, but did the existence of that test indicate that anybody knew anything about gum?

A. By "gum" do you mean resinifying products?

Q. Certainly.

A. That test does not particularly specify it, but this test of mills particularly specifies resinifying products.

Q. That is, resinifying with sulphuric acid, is it not?

A. But on hydrides containing less hydrogen.

Q. Is it not a resinifying material?

A. Yes.

Q. And it will polymerize, and it will happen with straight olefines as well as diolefines?

A. He says, "Resinifying and polymerizing."

Q. Yes. You can polymerize straight olefines; you do not have to have gum forming diolefines in order to polymerize, do you?

A. No, but if you had them they will polymerize the diolefines. They will polymerize easier than the mono-olefines.

Q. Yes, but Mills does not indicate that he knew there were diolefines or gum forming constituents on exposure to air, does he?

A. He knew he could purify his shale oil distillation by treating with sulphuric acid from ordinary products.

[fol. 600] The Master: From resinous forming products?

The Witness: Yes.

Mr. Wiles: Well, resinous forming, your Honor——

Q. Let me ask this: Those are products that form resins with sulphuric acid and not necessarily form resins on exposure to the air; that it correct, is it not, Doctor?

A. He is after the question of finishing his illuminants and lubricants in such a way that the oil will keep its color. He is after color, and the resinous products are, in general, color.

Q. And there are plenty of others that are colored, too, are there not?

A. You mean yellow dies and such things as that?

Q. No, I man heavy oils, all sorts of things are colored, are they not?

A. Yes. Yes, I think most of these are also products which, on sulphuric acid treatment will give us a tar product.

Q. Why, of course; we do not dispute on that. In other words, the formation of the——

A. I may add that they, in general, carry unsaturated compounds, too.

Q. Certainly; of course they do. Therefore, the formation of such a gummy mass from sulphuric acid does not indicate, necessarily, the presence of any material which forms gum on exposure to the air.

A. We normally think that the color forming and the resinifying products are of the same general character, and he says that this is done in order that the products may be of good color and that they will keep, the color will keep.

Q. Where do you see that?

A. I am reading on page 32.

Q. Just read it.

A. "Crude light oil and blue oil are treated with acid at about 13 degrees. A temperature of 22 degrees, however, does not injure the blue oil, and the tar then separates more easily; but the acid is then of less value for treating the crude oil. In finishing both illuminants and lubricants the temperature should always be low with acid. With soda it is the product—after stirring, particularly with 3 to 4 degrees Twd. soda—to steam the mixture to 54 degrees, the oil keeps color better in this way than when finished cold."

He also states, in the paragraph just preceding:

"That the vitriol used ranges from specific gravity 1.3 to 1.83; the soda from 1.05 to 1.3," which is the acid being used and the soda being used.

[fol. 601] Q. Will you please tell me where you find a disclosure in this art of the fact that you can take out the diolefines without taking the olefines with them, or that you can pick out those ingredients which form gum on exposure to the air without taking the olefines as well—any substantial part of the olefines as well and do that to the ordinary gasoline?

A. I am reading to you from Thorpe and Young——

The Master: It does not say a word about diolefines, as far as I can find, and I have searched through it now three times.

Mr. Wiles: No, but the witness has stated many times here that the diolefines are these same gum forming ingredients, and I put it in the alternative.

A. I find that such a process is described, though the purpose is not given, in Thorpe and Young, proceedings of the Royal Society of London, Vol. 21, page 188 of 1872:

"After repeated distillations over sodium, which portion was almost completely resolved into three fractions—"

And then he goes on and shows the olefines that he gets and they were unaffected by the process; on the other hand, he has, as we know from our general knowledge of diolefines, remov-d the diolefines.

That is one citation that does it.

Q. Please tell us which of the various references that have not been so far discussed, that is, all except Cabot and Thorpe and Young, disclose a product or material which, just as it is without change, fractionation, or addition, would have been suitable in 1913 for general use in automobile engines as a fuel therefor?

Mr. Wiles: I am not sure whether my point is clear to the Master. My position is Ellis' product is a finished, complete product, ready to go right into a motor and be used. That is what he has claimed.

Q. Which of the references in the art have a motor fuel likewise fit and ready to go right into a car and be used?

The Master: That is true enough, but if you are talking of a product of the process of cracking as it comes from the still, fit and ready to be used, compared with Ellis's product, that is one thing. If you are talking of a finished prod-
[fol. 602] uct, which is the result of refining with sulphuric acid, that is another thing. He starts with gasoline; Ellis starts with gasoline. His product is the result of refining gasoline.

Mr. Wiles: In a certain manner.

The Master: In a certain manner.

Mr. Wiles: A certain gasoline in a certain manner.

The Master: A certain gasoline in a certain manner.

Mr. Wiles: Now, the witness is taking two points.

The Master: If the witness can point to any such process of refining a gasoline, which brings a products within the Ellis patent, why, you want him to put his finger on it?

Mr. Wiles: Oh, yes; and the witness has taken two positions. That is why I have stated my question broadly.

The Master: Does not your question limit him obviously to the products which give the result as it comes from the still?

Mr. Wiles: No, the product in any stage in which it is set forth in the reference. That is what I want him to answer on; any product that is set forth in the reference.

The Witness: I think I understand you.

Mr. Wiles: But just as it is, not changing or additional refining, not set forth in the reference.

A. Bassett, "Petitioner's Exhibit No. 110," describes a product made of five parts light petroleum spirit, 79 deg. Baumé and one part coal products which should be rectified, of 28 to 30 degrees Baumé. Such a product, without change, would be a motor spirit, which would be such as has been described by Ellis.

The Master: Might I ask what the commercial name of the Ellis motor spirit is on the market, any of it?

Mr. Wiles: Motor gasoline.

The Master: What?

Mr. Wiles: Motor gasoline.

The Master: Is it sold by anybody under some special brand? Can I buy any of it on the market?

Mr. Wiles: Why, yes, buy some Standard motor gasoline right across the river in New Jersey and you will get it.

The Master: What is its brand? What is it called?

Mr. Wiles: Standard Motor Gasoline.

Mr. Rauber: You mean that is made exactly by the Ellis process?

Mr. Wiles: It is the Ellis product; that is what the Master asked me.

[fol. 603] The Master: What do they call it? What does the filling station call it?

Mr. Wiles: Standard Motor Gasoline.

Mr. Rauber: Any kind of gasoline, as far as that is concerned, straight run, any cracked gasoline.

Mr. Wiles: It is not a straight run product, what the Standard Oil Company of New Jersey sells on the Jersey side, and some of it on this side, sometimes. As I understand it, I am not thoroughly up on the exact methods; but as I understand it the regular product of the Standard Oil Company of New Jersey is the product.

The Master: What do you ask for out in God's country?

Mr. Wiles: Red Crown, and you will get as near as you can to it. Now, there may be, as I said in my answers to interrogatories, a most careful and minute chemical an-lysis that would take us years to find out, might demonstrate some differences between the Ellis and the Red Crown; but so far as we now know, Red Crown as sold by us is the Ellis product. If I were sued on it I would put in two or three years of careful research to see whether there were minute differences that have not yet been discovered that would differentiate it.

The Master: And your Solite differs from your Red Crown?

Mr. Wiles: In the cut.

The Master: Do you consider Solite as corresponding to the Ellis product?

Mr. Wiles: I do not know. I do not know Solite as well; I could not tell you.

The Master: It is the higher test.

Mr. Wiles: Yes. Well, the Ellis is particularly directed to a somewhat heavy fuel, and that is why I hedged on it—why I say I am in more doubt as to the Solite. I would want to make a very careful examination, more careful than I have made of Solite before I could answer that question.

Mr. Rauber: Your Honor, it seems to me that that illustrates one thing that Mr. Wiles asked Doctor McKee, to compare something he does not know himself with the prior art.

The Master: It does not make any difference whether Mr. Wiles knows what he is talking about or not, if he asks the questions.

Mr. Wiles: I am only counsel. I can be just as wrong as I choose, but the witness cannot. He is up against it if he follows my error.

A. (continued). "Petitioner's Exhibit No. 98, Engler and [fol. 604] Schneider of 1897, on page 2 of the translation, after discussing the cracking process, states that, 'from the different main fractions (whose boiling points were concentrated by the aid of dephlegamators * * * *.')"

This phrase "boiling points concentrated by means of dephlegamators" shows that is was fractioned in some kind of a column still, or its equivalent.

We would have some of the fractions here which would be the same as that motor spirit described by Ellis.

Q. I do not understand that, Doctor. Do you mean some of those fractions when combined or that there would be certain fractions that were finally made which would be suitable?

A. No, not finally made but intermediately made, because they have been fractioned into the individual constituents. But I am referring to the main fractions as he speaks of here, which would have, say, boiling points within the gasoline range and not within a kerosene or gas oil range; that is, a product which would meet the Ellis description.

Q. Just which main fraction did do that?

A. He says that he has fractioned here——

Q. Where?

A. (continued): —Into main fractions and then, later, he makes his separations into individual——

Q. Where does it state that?

A. Near the bottom of page 2, in the lines that I quote:

"From the different main fractions (whose boiling points are concentrated by the aid of dephlegamators * * *)."

I do not know whether you have got the same translation as I have or not. I will give it to you in the German.

Mr. Clark: That is the same translation.

Mr. Wiles: I do not see it.

Mr. Clark: Just below (indicating).

Q. Which are those main fractions? Are they not the ones just above given?

A. I do not understand so.

Q. Well, what were they?

A. They were the fractions. If we take his crude oil and distill it, and then from these he separated here his pentane, as shown on the following paragraph, pentane, hexane, octane, nonane.

Q. Are not the main fractions where he talks about those fractions below 100 to 120, 120 to 140; 140 to 160; 160 to 180, and so forth, as given in the table, 12 so-called main frac-
[fol. 605] tions; are not those the ones that he is referring to, and if not, what are they? What are the main fractions?

A. I do not understand that they are the main fractions to which he refers, but even if we want to take it as the fractions to which he refers, the product, for example, 100 to 120, would be a motor spirit within this description of Ellis.

My own understanding is that he has broken it up here into main fractions; that is, one fraction we will say of 100 to 200, and another from 200 to 300, and the third over 300.

Q. Does he describe that?

A. He has not described in detail these main fractions, but I said here that that was my understanding of the text; that he had made them into certain main fractions, as he speaks of main fractions, and that the one which would have the boiling points, such as I have indicated, would be a good motor spirit within the description of Ellis.

And, further, that if we even wanted to take it in a narrow form, that that narrow fraction, which I would not call the main fraction, 100 to 120, would be a fraction which would meet the description of Ellis.

Q. And that would be suitable for general use as fuel in automobiles of 1913, in your judgment?

A. It is a product described by Ellis, but it would not be suitable—this narrow fraction of 100 to 120, would not be suitable at certain times of the year.

Q. Would any of the twelve fractions that are specifically described in that patent?

A. No, but if you would take——

Q. All right, that is all. If you add something to them you could make them.

A. Wait a moment. None but the first fraction would be, probably, universally useful.

Q. You think the first one would be universally usable as a fuel in the motor cars of 1913?

Mr. Rauber: How do you get "motor cars"? You said "motors."

Mr. Wiles: That is the question.

A. Yes, it would be. You could run our automobiles of the general character of 1913 with a fuel of this fraction. It would not be a desirable fuel, first, because it would be too expensive; and it would be undesirable, again, because of the losses from evaporation, which would occur in there and have risks if somebody was riding in the car using a cigarette, for example.

[fol. 606] The Master: Do you know that that fraction that you just designated has an iodine number not less than 30 and as high as 75 or 100, is substantially free from olefines, readily forming resinous bodies on exposure to the air?

The Witness: It would have an iodine number as high as you have indicated. Yes, but the very fact that it has been dephlegamated and kept hot and distilled here at this time, one familiar with the light boiling products, would expect it to be free from resinifying bodies, after it had been so distilled and treated as it has been here.

A. (continued). Boynton, United States patent of 1886, "Petitioner's Exhibit No. 109," on the second column of the second page, describes a product in which naphtha, benzol and gasoline, the naphtha and benzol being from gas tar, that is coal tar, but have first been taken out from the tar itself; that these products are mixed with gasoline, and that this product may be used for carburetting gas.

He further states "these may be used after being simply but thoroughly mixed, but it would be better to re-distill them together before using, stopping distillation at about 350 degrees Fahrenheit."

Such a product, after re-distillation with an end point of 350 degrees Fahrenheit, would be suitable for use in a motor car, and indeed, we have similar mixtures used at the present in motor cars.

Q. What would its iodine number be?

A. I do not recall that in your question there was any specification——

Q. I did not. I just interrupted to ask that question.

A. Its iodine number would be low; its unsaturated number would be high.

The Master: Would its iodine number be below 30?

The Witness: I think so, but it would be——

Q. How much below, please?

A. How is that?

Q. How much below 30?

A. Distinctly, decidedly below. Oh, I would expect it to be around 15; 12, 15, 18.

Q. You testified in your direct before you made the tests at Mr. Neave's request, that would have a much higher iodine number, did you not?

A. I do not think so. I spoke of it as an unsaturated product.

Q. All right, that is all.

[fol. 607] A. (continued). For I have my own notes here, and in my own notes I said, "I cannot say just what its iodine number would be, so cannot state regarding certain descriptions, particularly referring to certain claims."

Q. Now, will you continue?

A. (continued). But it is, of course, highly unsaturated and would be essentially free from resinifying bodies due to the way it is made.

The Master: If you were trying to select a motor spirit having a substantial amount of unsaturated bodies, how much would you conisder a substantial amount of unsaturated bodies?

The Witness: I should say 20 per cent or more, and this product would have probably 70 to 80 per cent, at least 60 per cent.

A. (continued). "Petitioner's Exhibit No. 111," Walker, United States patent of 1910, describes a product made by cracking and which is said to be known in the refineries as gas-naphtha. It is a product which is stated to contain 40 per cent olefines and about 60 per cent of the saturated paraffins. Its boiling point ranges from 35 degrees to 125 degrees Centigrade. That would be 95 degrees Fahrenheit to 257 degrees Fahrenheit.

It is stated that it is believed the olefines range from 5 to 8 carbon atoms; that they are primary in structure.

If we take the average one of these, say, the 7-carbon atoms product, we would have an iodine number of 100 or possibly a little more.

This petroleum cracked product, taking as we are only the low ends, that is, 35 to 125 degrees Centigrade (95 to 257 Fahrenheit) would be without change, capable of being used in the motor cars of 1913.

Q. For general use?

A. For general use. It might and probably would carry some color, but as I understood that was not objectionable as indicated in the question.

Q. In passing, will you tell us how it would run in gum forming constituents?

A. Being such low distillate, that is, being the low boiling fraction, it would probably run sufficiently low in gum forming constituents so as to be ready to be used. Just the amount of gum forming constituents that it would have would depend upon the character of the fractionation of the distillation. It could, of course, be cleared of these by the [fol. 608] ordinary process, which had been known for forty years previously, if desired, of this color and resinifying products by use of sulphuric acid.

"Petitioners Exhibit No. 113," being an article by Allen of 1881, describes certain "representative commercial products" available in England (as this is an English publication) and one of them is gasoline from shale. This is a gasoline of high test, being one having a gravity of .665; that is approximately 80 degrees Baumé. The commercial product of that time, as I have quoted from the various authors describing the shale petroleum, were made by treating with sulphuric acid, and such a product would completely meet any of the specifications of Ellis.

It has a high iodine number; it is high in unsaturates, it would have an iodine number of about 100; it has a bromine number of 67; it is highly unsaturated, it is a high test gasoline, and from the fact that it was a representative commercial product of that date, we would expect it to be light in color, or practically colorless; and the fact that it was of high test, and 80-degree Baumé, would mean that we would have very little in the way of impurities to be removed by the ordinary refining process. So that the ordinary refining process would do it completely.

Q. And that, in your judgment, would have been suitable for general use as a fuel for the automobiles of 1913?

A. It would. It is too expensive a product, being too high test.

Q. Cost aside?

A. Cost aside it would give high evaporation loss and there would be, possibly, danger to the man if he struck his match if the container was leaking, but automobile tanks are ordinarily not supposed to leak gas into the driver's seat or near the driver's seat.

Q. Did you intend to answer my question in the affirmative or in the negative. Cost aside, would it have been suitable for general use in the automobiles of 1913?

A. I would say it would be, through not the most desirable fuel.

The Master: Would it have any substantial amount of stable unsaturated bodies?

The Witness: Yes, as Allen's analysis shows here, that it would have quite a large percentage, probably around 40 per cent. I have not figured it exactly; it may be 35 or it may be 45.

[fol. 609] The Witness: Yes, as Allen's analysis shows here, that motor spirit contained be of the aliphatic series?

The Witness: They would.

A. (continued). Williams, in "Petitioner's Exhibit No. 114," publication of 1885, describes a product obtained as a by-product in the Pintsch gas system, which he has collected from some seven different stations. Such a product, carrying as it does from 24 to 65 per cent of benzine and toluene, and also presumably additional amounts of aliphatic, unsaturated hydrocarbons, would meet the Ellis description; and, judging from the gravities given, would be available and

useful in many, but perhaps not all, the automobiles of the 1913 date. It would depend upon the efficiency of the carburetor. There would be some of the descriptions of Ellis that would not be met, but it would meet other descriptions.

Q. Which ones would it not meet?

A. Well, I would say that it would not meet, for example, Claim 15, where it requires a major portion of saturated petroleum hydrocarbons, which is the minor portion of unsaturates.

Q. How about gum?

A. After he has distilled it as he has here and made these separations, it would probably be essentially free from gum materials.

Q. Wait a minute. I do not get you on that. As I understand it, he has seven different specimens of practically Pintsch casinghead liquid; that is, liquid thrown down in the compressors of a Pintsch plant. Now, it is your position that any of those seven in that condition as he got them for test, met the conditions of my question?

A. I think on re-reading this that I answered slightly incorrectly. When I spoke of the low gravities I was thinking that this was the product after distillation. This was not, this was the original product, and as such probably carried some gum in it. But going back to the middle of the article——

Q. Just let me——

A. (continued). He discusses distilling then.

Q. I will go ahead with the distillation, but speaking of the seven original specimens, is it your position that any or all of those seven would have responded to the terms of my question.

A. I think that they would not be satisfactory general use products, because they would probably contain some [fol. 610] gums and so on, which would have to be removed by distillation, and above that he speaks of distilling the products.

"I first pass the vapors of the hydrocarbons through a powerful rectifying column——

And he then separates them into the parts.

Q. Now, he made a lot of cuts out of all these seven, of each of the seven?

A. I understand that he only made two cuts.

Q. How many?

A. Two, the cut point being at 66 degrees Centigrade; the portion boiling below 66 he uses for preparation of olefines, which he says he requires for his conversion into hydroiodates in order to continue certain work that he had commenced previously. As I said, this I do not think of as a general disclosure.

Q. Neither one of his cuts was suitable for motor fuel, after he got done in there; his raw material or either of his cuts?

A. His earlier cut, his lower cut would be a product fairly satisfactory. It would be a fairly volatile product, and as such, be troublesome possibly in the summer time.

Q. That is the cut below 66?

A. Yes, but otherwise than for that, that cut would be a satisfactory motor fuel.

Q. Suitable for general use in the motor cars of 1913?

A. Yes, it would be counted as a very high test gasoline.

Q. And the second cut would not be useful?

A. No, I am not certain, Mr. Wiles, as to whether the carburetors of that time would handle benzol.

The carburetors at the present time, most of them, will handle benzol, and if they would, then this second cut would be. But whether, in 1913, they would do that, I do not know. I am not familiar enough with the history of the development of the carburetor.

Q. Was this seond cut pretty nearly straight benzol?

A. Benzol and toluene, very largely.

Q. Now, of course, that freezes in cold weather?

A. Not a mixture of benzol and toluene.

Q. As found in commercial proportions from the Pintsch gas?

A. No, it would not freeze in cold weather.

Q. What would its freezing point be?

A. Oh, well down, because I know that we were getting shipments of benzol which carried relatively a small amount [fol. 611] of toluene, perhaps fifteen per cent, and we had no trouble; left the drums out in winter weather and they never froze on us.

Q. Now, when Williams made this cut in the two fractions, what became of the gum?

A. The gum would be left, and——

Q. I mean, of the gum forming constituents?

A. That would be left at the bottom of his powerful rectifying column, as he calls it.

Q. Wouldn't those ingredients distill over?

A. Not as you would carry it out in the laboratory, with the slow distillation. The redistillation would have the effect of polymerizing the gum materials.

Q. One of the functions of redistillation is to polymerize those gum forming bodies, and remove them—isn't that true?

A. Yes, I would say so, particularly when it is carried out in such a case, with a rectifying column, where you would have the column distilling in various fractions—perhaps a total of 200 times.

Q. That has a most useful effect in removing the gum constituents, has it?

A. It has a useful effect.

Q. Gum forming constituents, I mean.

A. Because when they are heated these gum forming materials may polymerize—that is, they are much more active and polymerize much more readily than the olefines, the low boiling olefines—that is, straight olefines.

Q. Then, it is a fact that the Lewis and Cook bubble tower must have a remarkable effect in this way—that is, it must take the cracked product and polymerize the gum forming ingredients, and give you a much better product?

A. It ought to give you a better product, and the Barbet, with its larger number of plates, would probably give you a still better effect, on that.

I would like to have the file wrapper of Lewis and Cook.

And, confirming this view I want to quote from the affidavit of Shaefer, in the file wrapper of the Lewis and Cook patent, in which he states, on page 3 of the affidavit, that "the bubble tower distillate treats and filters to the necessary color, without difficulty."

Whereas, that using the radiator still—that is, a still of the ordinary type, does not do so. We have, here this question of reaching the desired color—the color being ordinarily said to be due to the same product as the resinifying. [fol. 612] This removal can be accomplished better in such a scheme as that of Williams, where it is run through a rectifying column.

Q. Now, I was led off on to this line because of your remarks on Williams. Will you continue with the original list, please?

A. "Petitioner's Exhibit No. 115," Allen, of 1910, states, in Vol. 3, page 108, of his "Commercial Organic Analysis," that:

"Naphtha from bituminous shale"—in other words, shale oil petroluem—"contains 50 to 60 per cent of unsaturates."

And that the remainder are saturates, with no trace of aromatic hydrocarbons.

On page 110 he gives the gravity and the amount of bromine reacting with a 100 grams sample.

Such a product would be useful, as this is stated to be a commercial product of England, and, as such, it would be expected to be white, and free from gum forming products, because it was sold at a time when it was being used for automobiles, and I don't think we had any evidence that there was any change made between 1910 and 1913.

Such a product would be useful, and was unquestionably used as an automobile fuel of general use, at that time.

Further, even the naphtha from American petroleum, which was sold there, was a product which would approximately be that described by Ellis. It runs, if we take the figure of 18 3/10th, which bromine number, and change that to an iodine number, we would have approximately 29 2/10th, which is within, roughly, the 30 mentioned by Ellis.

So that we have even American products being sold in England, at that date, 1910, which were such as would meet the description of Ellis. And it must be borne in mind that these were commercial products, sold for this purpose at that time, 1910.

Vittenet—"Petitioner's Exhibit No. 117"—of 1903, "has for its object a process of chemical purification and deodorization of petroluem oils and benzine."

The method illustrated is to take petrol, which would be a gasoline, and which is the name under which gasoline is sold in France, and treated with sulphuric acid of 60 degrees Baumé, which would be about 77.6 per cent sulphuric acid.

This product is, then, after being treated with this same strength of acid as Ellis uses, is treated with caustic soda, which Ellis also uses, and is then dried, the resulting product being one which he says has a perfect clearness.

[fol. 613] He says the process may be repeated, after the first treatment, if a higher degree of purification is desired.

If our original crude "petrol"—gasoline—was a crude gasoline obtained by cracking, this would be Ellis's process of purification, and the product would be identical with Ellis's product, and would undoubtedly serve as a motor spirit for use in cars of ten years later—that is, 1913.

Q. What would be the iodine number of that product?

A. I don't know. It would be, if it was a cracked gasoline to start with—it would be identical with the iodine number of Ellis, starting from the same, or similarly cracked products.

Q. And if you started with a straight run, as you could, within the disclosure, then what would it be?

A. It would be probably 15, or something like that.

Q. Although there is nothing in this publication that indicates the Ellis iodine number one way or the other?

A. It is possible to operate the process either the cracked gasoline, as would be obtained at that day, primarily, as a by-product, probably, in the making of kerosene, of from a straight run crude gasoline—either one.

Q. Now, I want to examine you further about that process in connection with the treating process product, but will drop that, now, and you may go on with your illustration.

A. Petitioner's "Exhibit No. 119," being an article by Armstrong and Miller, in the "Journal of the London Chemical Society," of 1886, describes the decomposition of petroleum hydrocarbons at high temperatures, with the primary object of getting gas, and this article is concerned with the by-products, in the way of liquid hydrocarbons, which can be, and were condensed during the compression of the gas. It states:

"The liquid deposited during the compression of the gas is wholly volatile in steam, when recently obtained; and consists of hydrocarbons incapable of being polymerized by sulphuric acid, benzines, and a relatively very small proportion of hydrocarbon, unattackable for sulphuric acid."

He states, a few lines later:

"On mixing either liquid with moderate dilute sulphuric acid—two volumes of acid, one volume of water—much heat is developed, and by its action the unsaturated hydro-

carbons are, for the most part, converted into compounds, which are not distillable in steam."

He says, further:

"The acid should be added in small quantity, at first, and [fol. 614] care should be taken in agitating. In working with large quantities it is desirable to steam distill, since the treatment with two to one acid is at an end, and to well mix the distillate with the stronger acid, as, owing to the viscid character of the product, it becomes very difficult to complete the conversion into polymerides."

The product obtained, after this first acid treatment with the two to one acid, which would be 76 per cent, or less strength acid, and then steam distillation would give a product which could be used as a motor fuel in automobile engines of the period of 1913.

However, this would be somewhat low for aliphatic saturated hydrocarbons. It would be high in unsaturated hydrocarbons, and fairly high in olefinic hydrocarbons.

It would be, I think, useful as a motor fuel in the period of 1913, because it was made—I may add—from a petroleum of low sulphuric content, being made from the petroleum of 1886 date, and most of the petroleums of that date, in England, were Pennsylvania, or similar petroleums.

Q. And that, you think, would have been a suitable fuel for the automobile of 1913, in general use?

A. I think it would serve, yes, and be a suitable fuel for the 1913 automobile. The 1913 automobile was one that was a pretty fair automobile.

Q. And this is the distillate that you are setting up—the intermediate product?

A. It is between the gas and the tar products.

Q. But the stage of manufacture where you say it would have been suitable, is the product after the first treatment, and before the second treatment—is that right?

A. Yes—the second treatment is for the purpose of removing still larger quantities of the unsaturated product.

Q. The intermediate product is the one which you say has the preference?

A. Yes, but I think, also, that the final product would do but perhaps not quite so well. It would not so closely meet

Ellis's description, for example—being lower in unsaturated products.

That completes my list.

Q. And you have covered all of the instances, in these references, that you know of, where there is a product, or substance, that would have been suitable for general use in the automobiles of 1913, as a fuel therefor.

Mr. Rauber: You mean, those that he knows, off hand? [fol. 615] Mr. Wiles: No, no—in this list that he has given. I want them all. I want to know the case against me, before I get to my testimony, if you please.

Mr. Rauber: I suggest that he might read the whole list through.

A. I have notes here regarding three other citations, which I will have to look up. I thought I had the principal ones.

Answering the question, I think I have, where all the details of both the making, or refining—or, at least, the final products—is disclosed. I have not taken those cases where the product is made and disclosed in one citation, and a method of refining it in another citation, and the two being brought together, giving a perfect motor fuel for 1913.

In other words, I have not taken those cases where the citation can be taken and, in the light of the art prior to 1913, a product could be made, such as the Dewar and Redwood patent.

Mr. Rauber: To the best of your present recollection, you mean?

The Witness: Yes, I said I thought I had.

Q. Now, with reference to the Ellis acid treating process patent, you cited, if I remember right, three references, Vittenet, French, Armstrong and Miller, and somewhere in the record you mentioned Redwood's book, but not in exactly the same place. That is in my memory, and in my notes, but it is not in the same discussion. Now, are those the references?

A. I mentioned, earlier, on direct examination, in connection with this Redwood, Armstrong and Miller, and Vittenet citation, and on my cross examination I mentioned Mills, as another.

Q. Now, as to the Vittenet French patent, you seem to draw something from the French that I don't get. The material treated is defined as "petrol," isn't it?

A. It is.

Q. And in French "petrol" means crude oil, does it not?

A. It also means gasoline. If you would go to a motor filing station, you would ask for "petrol."

Q. Yes, you do now, but in 1903, what was the fact?

A. I think there were no filling stations at that time. Petrol would be the name for the gasoline product. I don't think that the name came in between 1903 and 1913.

Q. When you turn to the first claim of this Vittenet patent, he is very explicit, is he not, with the substance that he [fol. 616] is treating, that it is crude oil—that is, he says, "Petrol"—I don't pronounce French very well, but he says——

A. I have a copy of the French here.

Q. He says, "Petrol bruts"——

Mr. Rauber: That is crude petrol.

Q. Well, "petrol bruts" is crude oil, isn't it?

A. I don't know. I would think that it would be just as equally, crude gasoline. I don't know that anybody ever wanted to take the crude oil, itself, and remove its odor.

The Master: What is the date of the patent?

Mr. Wiles: 1903, your Honor.

Q. Now, when you read the title, it says, "The procedure by chemical purification and deodorization of oils and distillates of petroleum and similar products," does it not?

A. My translation reads, "Chemical purification and deodorization of petroleum oil and benzines and similar products." Now, benzine would be crude gasoline.

Q. I am asking you to refer to the original French.

A. I note, further, that for other treatment, with calcium chloride, it specifies that it shall be perfectly clear. Crude oils would not be perfectly clear.

Q. The original French is "de oels"—but I can't pronounce——

A. Oils.

Q. Oils and essences de petrol.

A. Essences de petrol would be gasoline.

Q. In the title to this patent the word, "petrol" appears.

Mr. Rauber: Essence of petrol.

Q. Oil and essences de petrol.

Mr. Rauber: Essences of petrol, as I understand, would be benzine.

Mr. Wiles: Well, that is not the question, and you are not testifying, anyway.

Q. Isn't it a fact that the word "petrol," appears in the title?

A. It does.

Q. And it speaks of oils and essences of petrol?

A. It speaks of the deodorization of the oils and essences.

Q. Of petrol?

A. Yes, sir.

Q. Now, the "petrol," there, must mean crude oil.

A. We don't speak of gasoline and crude oil as one,—as the translation has it, deodorization of petroleum oils and benzine.

Q. But the claim is very clear, that he is dealing with——

[fol. 617] The Master: Have you agreed on the translation of that French patent?

Mr. Wiles: I don't think so.

Mr. Rauber: I understood that they have accepted my translation of that.

Q. Will you look at the two claims in the original French, and tell me if it is not a fact that one claim is on the treatment of crude oil—crude petroleum—and the second one on the treatment of all other products than petroleum—"than the above"?

A. I would say that the claim of the French—the first claim of the French—if read out of connection with the specification, might be taken as of crude petroleum.

But, it, also, might be equally taken as a crude petrol—that is, of crude gasoline—and that, in the light of the title, where it says that it is for the chemical treating and deodorization of the "essences"—it cannot mean anything else than a crude benzine, or crude gasoline.

Q. It says not only essences, but the oils of petroleum?

A. Yes.

Q. That might mean the crude—and that is what the claim says, doesn't it?

A. The claim, I say, might mean that. And there is one other place which I think helps in this matter. While I cannot translate the French—there are some words that I get stuck on—but in the translation——

Q. As it is in your own?

A. I am speaking of the second place.

Q. Isn't it a fact that your own translation, that you are reading from, says "the process of purifying crude petroleums";

A. That is correct, and I have said that the claim covers that.

Q. That is all on that point.

A. But, on the question of it stating here with reference to the perfect clearness, as to the product obtained by the process when working on crude gasoline, I would say that it could not be—in the illustration—a process of purifying crude petroleum. And, moreover, nobody wants a process for purifying crude petroleum, compared with what they want a process for——

Q. Isn't it a fact that crude petroleum is frequently treated—acid treated, for cylinder stocks?

A. Yes, but not with perfect clearness, as he specifies, but it is, after you have removed your gasoline and kero- [fol. 618] sene. You do not ordinarily treat, but you remove your gasoline and kerosene.

Q. You can treat any time you please.

A. You are treating a residue—you are not treating crude petroleum.

Q. And when you treat for cylinder stock, you treat to make it clear and bright,—to make what you call bright stock, as distinguished from the ordinary residuum?

A. You are purifying it for——

Q. Yes—so that it is common to treat crude petroleum with acid?

A. No. You said a moment ago that you are treating a residuum, after you have distilled out the kerosene and gasoline.

Q. Isn't it a common practice, to your knowledge, to treat crude petroleum with acid, for the purpose of subsequently using it in the manufacture of cylinder stocks?

A. I don't recall any case in which crude petroleum, before the removal of gasoline and kerosene constituents, was treated with acid to aid in the quality of the cylinder oil.

Q. Can you say that that was not done, and was not common?

A. I cannot say that it has never been done. It is impossible to answer such a question in the negative.

Q. Can you say that that was not a common practice in the art, and well known to those skilled in the art—that is, the treating of crude oil or crude petroleum with acid?

A. In 1903?

Q. Yes.

A. I feel quite certain that it was not, but I cannot say, positively, that it was not, at that date—22 years ago.

Q. And how about it today?

A. It certainly is not common.

Q. Not common enough to be generally known by those skilled in the art?

A. We normally remove the kerosene and gasoline before it.

Q. That is not the question. Is it common enough to be generally known to those skilled in the art?

A. I don't recall a single case which has come to my knowledge, where the original crude oil is treated with acid as a first step. There may be such cases, however.

Q. Now, going to this Vittenet patent—I believe you said—and I want to be sure that it is in the record—that so far as this patent shows, the raw material may either be highly [fol. 619] unsaturated, or not—that the patent is perfectly silent on that subject—is that true?

A. That is correct.

Q. Now, furthermore, you have said that Vittinet treats with sixty Baumé acid—that is correct, isn't it—that is your statement?

A. Yes.

Q. Now, what he really does is to treat, first, with sixty degree Baumé acid, and then, immediately thereafter, by sodium bisulphite, is it not?

A. Yes, and then with caustic soda.

Q. Yes, neutralization is always present in any treatment—or should be. Now, he also says that this double treat—first with sixty gravity acid, and then with sodium bisulphite,—is the equivalent of a single treat with ten kilos of concentracted acid to a hundred litres of oil—that is about 34 pounds of concentrated acid to the barrel—

isn't that true? Check my calculations. I am never sure of my translation from the decimal system to the English. I figured on a 42-pound barrel.

The Master: Why do you compare it with barrels? Why don't you ask about gallons?

Mr. Wiles: Because, treats are always spoken of in pounds per barrel. A barrel is 42 or 50 gallons. They always speak of the number of pounds to the barrel, in the extent of the treat. They use five, seven or ten pounds to the barrel, of sulphuric acid.

A. Figuring on 42, I get a somewhat different figure. I get 36 and a fraction, instead of 32. I want, though, to raise—one supposition that you raised in your question, I don't think is quite correct. It is not given as an equivalent. It is given as a variation, and I don't know whether the two products resulting, would be the same, or not.

Q. Doesn't the patent substantially say you get the same result, either way?

A. My translation says, "Finally, by way of a variation." I don't happen to have a copy of the original French.

Q. If you read a little further along, it says, "Finally, by way of a variation; these two successive treatments can be replaced by a single one."?

A. That is what I have, but that doesn't necessarily mean that the two are identical products. That is my point.

Q. Isn't it the plain implication that they give the same result—one replaces the other?

A. I would say that one is, as it states, a variation of the [fol. 620] other—using the same material, getting somewhere nearer the same product.

Q. Now, now, 36 pounds to the barrel is a pretty heavy treat, isn't it?

A. Oh, yes, but we had that, for example, in the citation I quoted from Mills—they were larger than the present day. All of this older work used larger amounts. They didn't do as thorough mixing and did not appreciate that a thorough mixing would enable them to get along with less acid.

Q. It, then, is a heavy treat—36 pounds to the barrel is a heavy treat?

A. It would be considered now, a heavy treat. It is, particularly if you used as thorough a mixing as is used

now. With a poorer mixing it might have the same result as the small amount used at present.

Q. Then, if these two methods of treating—the one with sulphuric acid, followed by a sodium bisulphite, and the other with concentrated acid—are they interchangeable equivalents? That is, the first one would be a heavy treat, or the equivalent of what we would, today, consider a heavy treat?

A. On the supposition that you make, that they are absolute equivalents, yes. But we know, of course, that the sodium bisulphite does not act in the same sense as the strong acid, when given very thorough stirring.

Q. Now, in addition to merely using this large amount of acid, Vittenet makes it even more energetic, by adding to it some potassium bichromate, doesn't he?

A. And he suggests the use of a thousandths part—that is, a third of an ounce to a hundred litres—it would be almost an insignificant amount, of course.

Q. Do you think that would not make any difference, at all?

A. It might make some difference. It was probably put in for the purpose of making a difference, though I think the amount of a third of an ounce, or, perhaps half an ounce to the barrel, is an insignificant amount.

Q. Now, if it had a function, it would make the treat more energetic?

A. Probably. When we use bichromate as an oxidizing agent, ordinarily we use it not in parts per thousand, but parts per ten.

Q. Well, it is an extremely energetic chemical for that purpose, is it not?

A. Yes.

Mr. Rauber: For what purpose?

[fol. 621] Mr. Wiles: As an oxidizing agent, is what he said.

Q. Now, turning to Armstrong and Miller, the effort was to treat out all of the olefines, was it not? That is, the object of the final process was to get rid of all of the olefines?

A. If we carry it out in a process, as I suggested, with its omission, yes; it was to remove the olefines in still larger amount.

Q. The aim of the composite of the process as a whole was to get rid of all of the olefines; that is right, is it not?

A. He certainly would not accomplish it by treating it with acid of the strength that he suggests here.

Q. He says he does, does he not?

A. He says "to complete the conversion of the unsaturated hydrocarbons into polymerides."

Q. Exactly. That precipitates them and removes them, does it not?

A. Yes, but we well know that such a process as he describes here, using diluted acid will not readily remove all of the unsaturates.

Q. Do you know that?

A. Yes, I think I know that from personal experience. That is, I have taken and treated products containing olefines in considerable amount, with sulphuric acid of approximately the strength that is added here, and you finally reach a stage where it scarcely darkens the sulphuric acid, and yet the product contains perhaps as much as—well, it would have an iodine number well over seventy.

Q. When you performed this operation that you have just testified about, did you treat in separate dumps, or did you put all your acid in in ample quantity and agitate, together with the oil?

A. I put my acid in in separate dumps.

Q. Yes, and if you had treated with all the acid at once you might have got rid of all of the olefines?

A. I thought I used sufficient acid, in the first place, to have gotten rid of all of the olefines, but I actually treated it a succession of times; several times. I do not remember how many.

Q. This publication, then, does not, in fact, accomplish what the author says it does, is that correct? Is that your present position?

A. Of the part that I quoted, I think he accomplishes it thoroughly; the second portion, which I think you misinter- [fol. 622] pret there, I do not think he accomplishes what you have claimed he infers.

Q. He says, "it completes the polymerization of the olefines," does he not, in the same sentence that you quoted your part from he continues by saying that he polymerizes —"completes the polymerization of the olefines."

A. But if you read the succeeding sentence, you will see that the steam distillate finally obtained has peculiar char-

acteristics of unpleasant odor, due apparently, to the presence of volatile products and the action of the acid. And we know that that is what an unsaturated product would have.

Furthermore, on the next page here we have him treating again with sulphuric acid, the product. I would say that in here the product, up to the point that I have mentioned, that is, after it had been treated by the two to one acid and distilled, there is no question as to his meaning. On the four to one acid, the thought of this man in 1868 was that he was removing, by polymerization, the olefines. He was actually, removing and getting his black color due partly to the diolefines.

Q. Then he was doing something that he did not know about and did not disclose?

A. He did not disclose as fully; he discloses his product; he discloses his process.

Q. Does he disclose his product?

A. Yes, he gets benzene. He gets out here benzene, crystallizes it out; he gets out his paraffins and discusses here his unsaturated product.

Q. Now, let me go back to the sentence that you quoted?

A. (continued). And his xylenes.

Q. What he says he does, "On shaking either liquid with moderately dilute sulphuric acid, two volumes of acid to one volume of water, much heat is developed and by its action the unsaturated hydrocarbons are, for the most part, converted into compounds which are not distillable in steam." Now, I want to take that step first. It is perfectly clear from that, that even in the first step, most of the olefines are removed, is it not?

A. I should say so; that was his thought. And of the diolefines, if he recognized their presence. Of course, we now recognize their presence.

Q. Now, if that is a fact, that is not the Ellis treat, is it?

A. His theory may be wrong.

Q. His results—he says his results are the removal of [fol. 623] most of the olefines. Now, Ellis treats to leave nearly all the olefines, does he not?

A. He is treating his product the same way Ellis treats it, and he is getting the same kind of a product?

Q. Now, wait a minute. How about the amount of the acid and permitting it to heat up. If you use lots of acid

and little oil and allow it to heat up, can you not get just what he says, that is, the removal of most of the olefines, which is exactly what Ellis tried not to do?

A. In order to avoid heating up——

The Master: You mean the extent to which you carry out the process?

Mr. Wiles: No, it is a question of heat. Ellis is an extent patent, your Honor. Ellis's proposition is this:

I have discovered that I can treat this material and take out only the diolefines and leave the olefines by using a small amount of dilute acid.

A. But in this case, answering your question, he states that he wants to cool the product during the process. For example, "in dealing with small quantities, a glass stoppered bottle is used, but for large quantities, it is well to use a copper can with a tightly fitting metal plug. There is then no danger of the vessel bursting and it is much easier to quickly cool the mixture."

Then, "the acid should be added in small quantities at first and care should be taken in agitating."

All of those steps are the regular conditions that a man uses in the laboratory to keep a product from heating up. He takes it in his bottle, he adds a small amount, he puts it under the faucet, handles it and shakes gently first until the heat is absorbed by the cooling water. This is just the ordinary process which is carried out in every university organic chemistry laboratory, and the method used is a similar method.

Q. Your position then is that this reference which tells you in words that it removes most of the olefines, is a complete disclosure of a process which says it does not remove the olefines?

Mr. Rauber: Does not remove any of the olefines?

Mr. Wiles: Which leaves most of the olefines there.

A. The process as described is essentially the same as in the case of the Ellis, but the inference there that the unsaturated hydrocarbons are, for the most part, converted by the sulphuric acid treatment was probably only an in- [fol. 624] ference from the amount of color obtained. We know that such a treatment with sulphuric acid, particularly in the presence of dolefines, will give a black colored sulphuric acid and will color the oil itself.

Q. In other words, this statement in this publication of the result is not true?

A. It is only roughly true. That would be my——

Q. It is not true at all, is it, when it says it takes out most of the olefines? You are insisting that it takes out practically none of them, are you not?

A. I am not I am stating that it will take out a part and leave a part.

Q. How much?

A. I do not know how much is there.

Q. What?

A. I do not know how much diolefines is there.

If you will tell me how much diolefines is present and how much olefines are present, I think I can answer your question but I cannot tell you how much ~~until~~ I am told that or given the composition of the original mixture.

Q. The Ellis process involves treating and the removal of but a small part of the olefines, does it not? Is that not the plain inference all the way through?

A. I think not. For example, he quotes here, "That in the fractions boiling—" I am quoting from page 2 of the Ellis patent, the process patent, No. 1,318,061:

"The products will carry iodine numbers running from 310 down to say stopping at 130 degrees Centigrade; 136," and yet his final product, as he describes it, is a product between 30 and 75.

Q. Why do you stop at 136; why not run down to his whole product?

A. Because in this case we have here in Armstrong and Miller, products which are even more volatile than 136. Perhaps we ought to stop at 100 degrees Centigrade, say with an iodine number of 218, and yet in his final product he normally has from perhaps——

Q. Yes. You are treating——

A. (continued). —an iodine number more than thirty, and ordinarily perhaps, below 75 or 100.

Q. Now, comparing Ellis's initial with his final product, his final product.

A. Well, I am.

Q. Now, comparing Ellis's initial with his final product, [fol. 625] what part of the olefines does he take out, as nearly as you can tell?

A. Judging from this table, he would take out considerably—well, he would take out fully half, possibly more.

The Master: And as I understand it, your position is this, Doctor: Recognizing a demand in the trade for a refined gasoline substantially free from resinous-forming bodies, Ellis took the Armstrong process of sulphuric acid treatment and got what one would naturally expect him to get from Armstrong's process?

The Witness: That would be correct.

The Master: And got it in greater or less degree, as he varies the steps of Armstrong's process?

The Witness: Yes.

The Master: Or his quantities?

The Witness: Or his time.

The Master: Or his time?

The Witness: Yes.

Q. When he did that he had to discover first that Armstrong and Miller were wrong on the statement that they took out most of the olefines?

A. No, I think not. I would say, in general, Ellis takes out, at least of his lower boiling products, the products corresponding to Armstrong and Miller; he takes out with the same——

The Master: He would take out more or less of the olefines as he varied the quantities or time?

The Witness: Yes. But he actually is not taking out probably any more—he is actually taking out very nearly the same proportion as Armstrong and Miller took out of that portion with the same boiling point.

Q. Your position is, then, that this reference which says that the olefines are, for the most part, converted into polymerize, which means remove, would lead one directly and without experiment to treating so as to leave the bulk of the olefines in?

A. If they wanted to.

The Master: In other words, had any one of these companies making gasoline gone to any chemist having experience in the art of refining petroleum products, distillates, and said to him, "I want my gasoline to be substantially free from resinous forming products. Tell me how I can do it or show me how to do it;" he would have naturally

turned to the Armstrong process of sulphuric acid to accomplish it?

[fol. 626] The Witness: The Armstrong or other of these citations.

The Master: Yes; or other of these citations by sulphuric acid treatment?

The Witness: Sulphuric acid treatment, small in amount or a particular diluted acid.

Q. Now, you also use Redwood's book as a citation of this diluted acid treatment. The fact is, as to the process disclosed by Redwood, is it not, that the shale oil with which he was dealing, was first treated heavily with concentrated sulphuric acid, and was then re-distilled and the distillate was then treated with dilute acid?

A. I was referring to page 431 of Redwood, and there is no evidence that it was treated previously with this acid of specific gravity, 1.720 which was mentioned; at least I find nothing.

Q. Look back a couple of pages and see if that same oil had not been previously treated by a concentrated acid?

The Master: I do not see why it is necessary to bring that out on cross examination when it can be brought out on the reference or on the argument.

Mr. Wiles: Sometimes the short statements is a better summary than the long one.

The Master: I have not discovered any short statements in this trial yet.

A. This I am quoting from is the first page of the "Petitioner's Exhibit," except the title page.

Q. What page is it?

A. 431 is where I was reading. At the bottom of page 431.

Q. Now, I wish you would tell us what the practical and commercial differences are, when working on a large scale, in practical refinery practice, between the use of dilute acid and the use of concentrated acid?

A. The use of concentrated acid permits a greater economy of acid though at a sacrifice of the kerosene or gasoline, which is being treated. The use of the dilute will permit larger yields; that is, less refining losses than the use of concentrated acid, but you have to use more dilute acid

and consequently the excess cost of that balances, at least in part, the additional yield of refined gasoline which might be obtained.

Q. Are those the only practical differences between the two methods of treatment that you know of?

A. No, they are not the only ones, but they are the principal ones.

Q. I want you to state, please——

The Master: Are you referring to mechanical differences?

Mr. Wiles: I want the whole effect.

[fol. 627] The Master: The necessities of stirring and all that sort of thing in large quantities?

Mr. Wiles: The whole practical effect on the practical industry of shifting from dilute to concentrated or visa versa.

Q. What determines and what are all the factors that determine what a big refinery, working on a big scale, is going to do and what it should do, strictly chemical engineer's advice, as to which one to use?

A. The primary, determining factor in answering any such question is which will pay the most profit. We have as the two items here concerned, the cost of an additional quantity of sulphuric acid, because we must use more if we use dilute acid, as against the income which may come from a less loss; that is, a greater yield of finished gasoline.

There are other secondary effects concerned: If we use strictly the strongest acid we can use, for example, iron containers and mixers. If, on the other hand, we are using a dilute acid it is necessary to use or to line our vessels with lead, and that is a relatively expensive addition to the apparatus. The putting in of sheet lead lining is expensive, and moreover, it is not permanent. They get out of fix rapidly and leak and give trouble and have to be continually repaired.

With the use of strong sulphuric acid, and meaning by "strong sulphuric acid" acid of approximately 93 per cent strength, so-called 66 degree acid, we would have normally a greater removal of the diolefines first, and of the olefines per unit weight of acid used.

[fol. 628] Approximately the same results would be accomplished by the use of dilute or concentrated. Both have been used for many years, as has been suggested by

the Redwood reference of 1900, and by the still earlier reference of Mills of 1892.

Another element that enters in on the question of the use of sulphuric acid is the expense of recovering that sulphuric acid, but as to this, using the present day schemes, there is comparatively little difference in cost of recovery per ton of pure sulphuric acid recovered as between the two. This was not true a few years ago. A few years ago we could not recover this acid, or at least it was not done practically at all; so that the question of the use of the larger amount of dilute acid is the then art of recovering sulphuric acid, was less desirable than it is at the present period.

There are probably other phases that enter in but I think those are the principal ones and perhaps all that you care for me to give.

Q. Do you mean by your statement as to recovery, that it is as easy to recover the acid as dilute, as it is to recover it as concentrated and vice versa?

A. Not as easy; some more fuel is used, but the amount of fuel is small, in any case.

Q. In recovering it in which way?

A. In recovering it, for example, by the——

Q. No, I mean as dilute or concentrated.

A. In either case it is not really a large amount.

Q. Which one takes more fuel?

A. It would depend somewhat on how dilute the acid was used in the dilute scheme, because when using in the concentrated scheme, using acid as a concentrated acid, it is normally diluted somewhat in order to separate the tar materials, which are then perhaps burned under the boilers, and the partially diluted acid would then be sent through the present-day acid recovery plant.

If, on the other hand, we use the diluted acid, and diluted it as much as suggested by Ellis, it might be even more dilute than, according to my understanding, is diluted when using concentrated acid.

The Master: What is concentrated and what is diluted; is it 50 per cent strength, 60 per cent, 70 or 80 per cent strength?

The Witness: Those are all diluted acids.

The Master: They are all diluted, are they?

The Witness: They are, but a ninety-three per cent acid would be counted as a concentrated acid, in this connection.

Q. I do not think you understood my question. At least [fol. 629] I have not had anything that sounded to me like an answer to it.

Which is the more expensive to recover, the acid as concentrated acid, or to recover it as dilute acid?

A. In present-day they are about the same.

Q. Well, you said in an answer a little bit ago that there would be some difference in fuel, you comparing those two; which did you mean would take the most fuel to recover as concentrated, or to recover as a dilute?

A. It would depend on the degree of dilution used, whether we used thirty or forty per cent or used eighty per cent.

Mr. Rauber: I wonder if you understand the question? (Addressing Mr. Wiles) Do you mean to recover a concentrated or to just leave it dilute?

Mr. Wiles: Yes.

Mr. Rauber: You mean to concentrate it?

Mr. Wiles: To bring it up to sixty-six gravity or only to seventy-five per cent strength. That is what I thought he referred to.

Mr. Rauber: I do not think he quite understood. I think he means starting with a dilute——

Mr. Wiles: You do not start that way, anyway.

Mr. Rauber: I thought he misunderstood your question.

The Witness: I think I understand the situation there, and I would say that it was approximately the same; because you have to heat it up high enough to oxidize part of your organic materials and then you would dilute it down again, so that it would be approximately the same.

Q. Was I correct if I understood you to say in the last few answers, that acid recovery was something new; was that your statement?

A. That acid recovery at the present day can be carried out much more cheaply than it could be a few years ago, and I will give you the reasons if you care for them.

Q. Now, having in mind the conditions in 1913, what were the differences, if any, in recovering your acid as dilute, and recovering it as concentrated acid?

A. I think the first sludge acid recovery plants of tower type, built with acid-proof brick and so forth in the refineries have been put in within the last four or five years.

Q. And before that what happened?

A. Before that there were attempts to do this but they were not anywhere near as cheap operating schemes as the present-day schemes.

[fol. 630] Q. You mean that they did or did not recover the acid before this?

A. I say there were operating schemes which did. There were other places where they threw it away, discarded it as waste material, and bought new acid.

Q. And in 1913, 1912, '11 and '13, what was the normal and general practice of the industry in America in that regard?

A. I think that the rather normal practice was a compromise between, where they took their product (and of this I can only give it in a general impression; I am not absolutely certain as to what was the average or majority practice) that they took the waste sludge acid, added some water, allowed the sludge to settle, separated the partially diluted acid out, and concentrated that by the common concentration processes of the sulphuric acid industry; and that the sludge acid, which was the other part of the sulphuric acid, together with a considerable amount of what is now recovered, was then simply discarded or used simply as a fuel. As I say, this is only a general impression of my own. I was not interested in the oil industry particularly in 1913, my interest, as I stated in my direct, began about 1917.

Q. And the American standard practice of 1911 and 1912, you do not know much about?

A. Only in a general way. I was not working in such a plant nor concerned primarily with it.

Q. Now, in considering the Dewar and Redwood patent, you stated in substance, I believe, that in that process gas would be pumped in only in starting up the process and not during normal operation. Please tell us your authority for that statement.

A. Before answering that question, I wish to say that I have now the copy of Mills which the photostat man had. Do you want me to quote that?

Q. I did not ask you anything about Mills.

A. (continued). As we were discussing the other Redwood in connection?

Q. I did not ask you anything about Mills.

The Master: Mills has already been on the record.

A. In the patent itself the statement is made: "That we charge and keep charged in the space in the boiler or retort and condenser that is not occupied by liquid with gas under considerable pressure."

I read also the same author describing his same appa- [fol. 631] ratus, but in 1896, page 322 of Redwood's Petroleum, beginning at the eleventh line:

"The retort, C, being partly charged with oil by the pump E, and the spaces in the retort, C, and the still head, C_2, and the condensing coil, D, and the column D_2 being charged with air or gas at the desired pressure;"

I notice that there is a semicolon at the end of that quotation.

Whereas, the other distinguishing portions:

"The retort is heated, vaporizing the oil under pressure."

In other words, a succession of operations is given by the author of the patent in describing it. Moreover, it would be very foolish for the purpose for what he wishes it, if he did anything else than to put in his gas at the start of the process under pressure. There is no occasion for putting it in later once the operation is working.

Q. Now, turning to the drawings, Fig. 1, there is indicated on the side of the receiving drum, D_2, a gauge glass, D_3; what is the purpose of that glass?

A. It is to indicate the height of the liquid in the receiving drum, D_2.

Q. And the level in normal operation is held within the limits of that glass so that it can be observed, is that correct?

A. I presume so; at least no higher than the top of the glass.

Q. No higher than the top of the glass, and as distillate comes over it is drawn off through the pipe, D_4, to keep the level somewhere in the glass?

A. That is my understanding.

Q. And that is a desirable thing to do in a cracking process, is it not, to watch your distillate level and keep it where you can see it?

A. In the operation you want to know what is happening in the process.

Q. If you did not do that you could not tell how much distillate you were making by metering, because of the fluctuations inside which you could not see?

A. That is correct.

Q. Now, in a system like this, operating under pressure, and with no gas being pumped in, as you have stated, during operation, there is some gas being formed, is there, all the time?

A. There is.

[fol. 632] Q. And the atmosphere in the receiving drum, in the lower part of the condenser, will be practically all gas, will it not?

A. Yes. Any atmosphere is a gas.

Q. Well, I mean gas as distinguished from vapors of the liquid.

A. Largely, yes.

Q. Well, if you were running, say, at a pressure of 50 pounds, with any pressure distillates that you are familiar with, and the receiving drum temperatures such as those you would normally expect, you would not have more than, say, three or four pounds absolute of vapor pressure?

A. If your cooling water was reasonably cool.

Q. Well, if you were running as a refinery ought to run, you would not have more than three or four pounds there, absolute, of vapor pressure?

A. I think that is correct. It is a little hard to calculate exactly.

Q. Well, somewhere in that order?

A. Yes.

Q. It would be well below atmospheric pressure?

A. Yes.

Q. And the remainder of your 50 pounds gauge pressure, or 65 pounds absolute, would have to be made up of gas?

A. That is correct.

Q. So that with 50 pounds pressure you would have perhaps 10 parts of gas to one of vapor or something in that order?

A. I think that would be correct.

Q. And you must maintain that condition; that is, enough gas, uncondensable gas, to equal the desired pres-

sure in the apparatus in order to hold your pressure; that is correct, is it not?

A. That is correct.

Q. There must also be enough gas to give you perhaps 60 pounds absolute of pressure in the receiving drum?

A. If you chilled the condenser D, say down to a cold-water——

Q. Well, around 60 or 70, as we normally would in a refinery.

A. I would say 60 degrees Fahrenheit.

Q. I am not trying to get absolutely accurate figures, but mere approximations of what we must expect. Now, gas is constantly being removed from this system, is it not?

A. Yes, and being produced in the system.

[fol. 633] Q. It is constantly being removed in solution in the distillate?

A. Yes, to a certain extent. Not particularly, for example, hydrogen, which is not a very soluble gas, which will not remove readily, but such a gas, say, as propane, which might be formed to some extent, would be readily soluble.

Q. The kind of gas that is made in the still is constantly being removed in solution in the distillates, is it not?

A. Part of it; the hydrogen scarcely at all.

Q. And there is not very much hydrogen there?

A. Well, there is always some.

Q. Yes, traces.

A. More than traces.

Q. 1 per cent of the total gas, or something on that order?

A. No, I think very much more than that. I do not recall the figures, but my impression is——

Q. There is not over 4 or 5 per cent hydrogen, is there; is that not the upset limit?

A. I do not find the figure that I thought I had as to the analysis of the gases from a cracking process. I do find some other figures in connection with these gases from cracking processes, but not so far——

Yes, I find:

"A typical gas made in a Burton still gives the following analysis:

"Methane and ethane 82 per cent; olefines 8.5 per cent; hydrogen 9.5,"

so that there is considerably more than 1 per cent.

Q. Where did you get that?

A. From Bulletin No. 15, Kansas City Testing Laboratory.

Q. Who is the author of that work?

A. Roy Cross.

Q. Is his work generally regarded as a standard authority in this industry?

A. I think that probably this bulletin is used in the way of a petroleum hand book probably more than any other, or as much as any other.

Q. That Cross is the same Cross as the patentee, or one of the patentees, of one of the patents you have discussed here?

A. I presume he is. It is a man of the same name and the same residence.

Q. Now, Doctor McKee, is it not a fact that in order to [fol. 634] maintain pressure in a system of this kind without introducing gas from the outside, the gas must be made constantly in such an amount as to saturate the distillate at the receiving drum pressure and temperature?

A. Yes, and to have enough left over to keep up the pressure.

Q. And it is your opinion, when you say that this process would be run without the introduction of gasoline from outside, that it at all times after the start makes enough gas to keep the distillates saturated?

A. Yes.

Q. Now, tell us the reason for believing that the gas is always made in sufficient quantities to saturate the distillates produced?

A. In the first place, it is only necessary to have an amount of our constituent made by the cracking operation itself in sufficient amount to more than saturate the condensate with that constituent. In other words, if we had more, hydrogen made per hour in the retort than was drawn off per hour through the condensate it would not matter if all of the other gases were dissolved by the condensate. The hydrogen is, relatively, a slightly soluble gas, the olefines, which are mentioned here, are probably much more soluble; meaning by that, ethylene and propylene; the methane and ethane are, to some extent, soluble, but we know many cases, where, for example, gas comes

off from oil even under high pressures, pressures of a thousand pounds, much less a pressure of fifty pounds gauge; which we have coming off at a thousand pounds from our gas wells, and from the oil from beneath it, which oil would be as good a solvent as this; in fact, a better solvent for methane than the light distillate would be. We have this gas coming off even at a pressure of a thousand pounds, so that it could not be very soluble, this methane, and to a less extent ethane, in the so-called natural gas wells. So that I really think that we have here, possibly, a question regarding the olefines, but scarcely any question regarding the hydrogen and methane, with possibly a little question regarding the ethane, though I am inclined to think there is ample of that as well.

Q. Now, what actually happens is, if there is hydrogen present, it will concentrate and build up in the space, will it not?

A. Yes, and methane or ethane.

Q. Whatever one; the least soluble gases will build up? [fol. 635] A. Those that are soluble will not build up.

Q. Exactly. And, finally, you will have a mixture that is just in equilibrium at that pressure with the liquid?

A. No, it will pass off as excess. I do not think it will be in equilibrium with the liquid, particularly, the excess pressure.

Q. Its composition will be just in equilibrium, will it not?

A. I do not know what you mean when you speak of the equilibrium, say, from hydrogen—a composition of hydrogen.

Q. Its percentage in gas; that is, the gas mixture that is forming the atmosphere in the upper part of the receiving drum will be just that which does not dissolve in the liquid at the pressure and temperature? In other words, it will be in equilibrium with the liquid for the moment?

A. If you mean it will be a constant composition, I think that is right. If you mean that it, by changing liquid, say by bringing your liquid into another vessel, you still get off that same kind of gas, I do not agree with you at all.

Q. No, it is in equilibrium with your liquid, is what I am getting at. That is, you arrive at a constant composition of gas which determines the pressure; which is determined by the pressure?

A. Yes, you arrive at a constant composition, which is determined by the pressure.

Q. And that will be richer in the less soluble gas than the gas originally formed?

A. Yes.

Q. Now, that is the very gas that Cross gave you an analysis of, that gas in which the hydrogen had become thus concentrated, is it not?

A. It is not. He says, "A typical gas made in a Burton still gives the following analysis —" not from the condenser or from the receiver.

Q. Is that not the gas released——

A. How is that?

Q. How do you suppose Cross got that sample?

A. I do not know, but whether he took a product of the still before it had been absorbed—a portion of it had been absorbed, I would presume he was saying what he meant; that is, that it was a product made in the still. That is what he says.

Q. That only place on a Burton still—this refers to the Burton process, does it not?

A. Not particularly, excepting that he speaks here:

[fol. 636] "The gases produced by cracking likewise are not simply split off hydrocarbons but vary according to the mode of cracking. In the liquid phase cracking the chief variation is in the olefines and hydrogen content. In a general way there seems to be a tendency for low percentages of hydrogen to be associated with low percentages of olefines.

A typical gas made in a Burton still gives the following analysis."

Q. All right. We will take, now, a Burton still; how are you going to get a sample off except through the high-line?

A. What is a high-line?

Q. Do you not know, as an expert in this industry?

A. It is a word that is used in various industries in different ways.

Q. Do you not know the nomenclature of the oil industry?

A. Only in part. I do not know all their slang phrases.

Mr. Rauber: You do not know how Burton uses it?

The Witness: No, I do not know how Burton uses it.

The Master: The word has never occurred here before.

Mr. Wiles: Well, by the "high-line," I mean the high pressure gas line shown in the Clark patent.

The Witness: Shown in the——

Mr. Wiles: In the Clark patent, which he said was an ingenious expedient for doing certain things in the process.

Mr. Rauber: He said it was what?

Mr. Wiles: He said it was an ingenious arrangement of pipes to accomplish certain purposes, and I agree with him entirely.

Q. Now, where are you going to get a gas sample out of a Burton still in normal operation except through the high-line?

The Master: You mean by "high-line," the high pressure——

Mr. Wiles: The high-pressure gas line.

The Master (continued): High pressure line which carries off the permanent gases?

Mr. Wiles: Yes.

A. He could have taken it off from where the vapors come, condensed them without putting them under pressure and examined the gas, and that is apparently what he did, because he says that is the gas made in a Burton still, not the gas obtained from the high-line, or not the gas obtained from the condenser.

Q. The high-line gas is gas made in the still, is it not?

[fol. 637] A. It is a part of the gas made in the still.

Q. Now, referring, again, to the Dewar and Redwood drawings—Dewar and Redwood released the gas that they made, out of the valve, D5, didn't they—the excess gas—out of the valve D5?

A. Yes.

Q. Now, I suppose you would say that the analysis of gas released from the valve, D5, was not an analysis of the gas made in the still?

A. It would be not the gas made in the still to the extent of the products which have been dissolved out of the gas—by his gasoline, or kerosene.

Q. According to your theory that gas ought to be nearly pure hydrogen—is that correct?

A. No, I don't think we would have anywhere near pure hydrogen. I would imagine that we would have relatively

little in the way of olefines, but a very considerable amount of methane, and probably a fair amount of ethane.

Q. Now, taking the Burton process, as you understand it, how would the gas analysis, of the gas released from the receiving drum, differ from that given by Cross?

A. I would expect it to be lower in olefines.

Q. How much lower—let us have the figures.

A. It would depend altogether on the oils used and on the temperature of your condenser and various schemes of operation.

It is impossible for a man to guess—and, moreover, it would be different from day to day, and from hour to hour.

[fol. 638] The Master: You are talking about the Burton patent, No. 1,105,961.

Mr. Wiles: No—667. I am talking about the Burton process, as actually operated. It is not exactly in any of his patents.

The Master: Well, if that is a standard that is in your mind, it is not on the record here, and I don't know how anybody could tell.

Mr. Wiles: Well, he saw it operating, and——

The Witness: To the extent of an hour and a half, in June, or July.

Q. Now, you have told us that the solubility of methane is very low. What is its solubility in any of the oils of the type of a pressure distillate?

A. I cannot give it to you in figures.

Q. Now, having again, reference to this Dewar and Redwood patent—it is necessary that they either make enough gas, just exactly enough gas to saturate the distillate all the time, or more or less—that is obviously necessary, isn't it?

A. That is correct.

Q. And the chance that they would just make the right amount is very, very small, all the time, is it not?

A. That is correct.

Q. If they are making more gas they have got to release it out of the valve, D5, all the time, to keep their pressure from rising?

A. Yes.

Q. Now, isn't it a fact that the Dewar and Redwood patent says that at times, only, gas is to be released?

A. It is spoken of as a safety valve, D5, from which gas may be withdrawn from the space above the liquid in the condenser, D_2.

Now, the safety valve would operate every time the pressure reached sixty pounds—if that was where it was set—and when it was operated it would release the pressure, perhaps down to fifty-nine pounds, and it would operate from time to time, as the pressure went up, and then went down, because these safety valves do not keep it at exactly the same pressure. They release the pressure and then the spring brings them back. There is a certain momentum of the gas which causes it to overstep at each release. He calls it a safety valve. I read from page 2 of the patent, line 45: "Safety valve, D5." And that is a suitable, loaded safety valve.

[fol. 639] Q. It is either a pipe, or a cock, or a suitably loaded safety valve?

A. Yes, sir.

Q. And that is shown in the drawing as a pipe and a cock?

A. Because the space, there, is small, and it would be difficult to draw.

Q. It is an alternate construction, and the drawings show a pipe and cock—that is correct, isn't it?

A. I think so, but he specifies a suitably loaded safety valve, D5.

Q. Or a pipe and cock?

A. Yes.

Q. Now, have you read the Dewar and Redwood British patent, described in this same process?

A. There is, I think, but I haven't seen it—I haven't looked over it.

Q. Does it refer, here, to that patent, in the American patent?

A. Yes, sir.

Q. The British patent states very plainly, doesn't it—oh, you say you have never seen the British patent?

A. No.

The Master: I don't think this is cross examination. I think this is bringing in a part of your case, Mr. Wiles.

The Witness: The British publication on the refining of petroleum, on page 322, uses the same words as the American patent. This is of 1896.

"By a pipe and cock, with a suitably loaded safety valve, D^5, gas may be withdrawn from the space above the liquid in the column, D^2."

Q. Then your position is that there is so much gas made in proportion to the distillate, that it has to be done all the time, practically, or at frequent intervals?

A. I would expect that this would be—that you could use a pipe and cock and set it to bleed the gas continuously, or by a safety valve, such as he mentions—a suitably loaded safety valve—that it would pipe off and release gas through the pipe beyond D5, from time to time.

Q. What I am getting at is that there would always be an excess of gas—plenty to maintain your pressure, without pumping in from outside, after you start up?

A. Yes.

Q. And that would be true at any pressure you chose to operate it?

A. If you would operate at pressures at which methane [fol. 640] is a liquid—say at 3,000 pounds per square inch—or something like that—it probably would not, but we would have to use pressures which would increase greatly the solubility or liquify the olefines there—not pressures, of around 50 or 100 pounds.

Q. Now, just how high a pressure could you carry this Dewar and Redwood patent process to—running for kerosene?

The Master: Isn't it stated in the patent?

Q. (continued). Without pumping in outside gas?

A. It would be dependent, as the patent, itself, says—I don't recall whether the patent definitely states, but it implies, at least, that it would be dependent, very largely, upon the raw material used, and the progress wished, as to the pressures and temperatures to be used.

Q. What is the upset limit—how high do you think you could go?

The Master: You can't tell, without ever having seen the apparatus, what way it is constructed.

Mr. Wiles: If he knows the cracking process thoroughly he could pretty nearly tell, your Honor.

The Master: It would depend upon the construction of the apparatus.

Mr. Wiles: I don't mean the strength of the apparatus, at all. The point of my question is, how high you could go without putting in outside gas. It is purely a chemical question.

The Master: How much pressure can you get in your system without reinforcing it with outside gas?

The Witness: It would depend upon your cracking scheme. I have here a statement that when, in the cracking process, ten per cent of the raw material goes to gas, than we are reaching a limit that is economical. Then if we were operating here, with an oil, which would crack and give us a total of nine or ten per cent of its weight, and with gases such as these—as given by Cross,—we would have the possibility of going to quite high pressures—probably—so far as the pressure is concerned—we would have trouble in distilling our kerosene, but as far as the pressure is concerned, and the gas given off, pressures in the neighborhood of 700 or 1,000 pounds.

The Master: By carrying up your temperatures in the apparatus of the Dewar and Redwood patent, you could develop in it a pressure of seven or eight hundred pounds?

The Witness: Yes, sir.

The Master: If it would stand it?

The Witness: Yes, without blowing up.

[fol. 641] Q. Without putting in any outside gas?

A. I think there would be sufficient hydrogen and methane to give us that.

Q. And that would be true, running for kerosene, as they contemplated in that day?

A. Yes, excepting the one condition that I gave——

The Master: They would not want to develop that pressure?

The Witness: Oh, no, they have no thought of developing that, because, to do so, would be to cause troubles in the way of carbon formation, and other things, and it would be useless to do it. They wouldn't want to do it.

Q. Then, it is your position that the Dewar and Redwood patent clearly discloses a process in which the introduction of gas from the outside is never necessary under their operating distillate level—which is with the distillate down in the gauge glass?

A. Yes, I think that is true.

The Master: Doesn't it come to this, that if they wanted to keep their temperatures down, without developing sufficient gas to largely raise the pressure, that they would have to get the pressure by gases, elsewhere?

The Witness: Your Honor, in such a case as that it would probably—if they were cracking at all, they would get sufficient gas to carry it out. Moreover, if there was any question in their minds regarding it, they have another way out of that—that is, to not cool quite so low. Then your solubility will decrease, if you simply cool down and use less water in your condenser, and allow it to cool, say to 90 degrees Fahrenheit, instead of cooling to 60 degrees Fahrenheit—the solubility of your gas would be very much decreased, and you would make one cubic foot of gas do the work of a good deal more.

Q. You know it is very bad refinery practice to run your distillate warm, if you can avoid it, don't you?

A. There is no particular reason why you could not use two coolers, one a condenser and the other a cooler, and that is often done in such factory distillations.

Q. Yes, but why should they go to all that trouble and run under bad practice and bad temperatures, when they have this pump with which to put more gas in it—to avoid doing what they say in their patent?

A. It would be just as cheap to do one as the other. It would be cheaper, even, probably, not to cool—because you are saving on your cooling water, as I say, and that is [fol. 642] another way. You have particularly specified here cooling down to 60 or 70.

Q. Normally good refinery practice, I said.

A. Well, you said 60 to 70.

Q. Well, that is good practice, isn't it?

A. It is good practice in winter time, and it is good practice, but in the summer time I don't thing it would be.

Q. You do it just as nearly as you can, don't you—you get down to ten degrees below your cooling water, all the time, in a good refinery job, don't you?

A. Ten degrees is excellent chilling.

Q. Doesn't a good refiner do it, on light distillates?

The Master: You are speculating a lot, in this matter,

Q. What does a good refiner do, in that regard?

A. If he is making kerosene, as Dewar and Redwood were probably, primarily, considering, more than gasoline, but they did get gasoline more than they wanted, and the process fell down.

Q. How do you know it did? You have said that twice.

A. Well, because it is given here—I quoted one place—the exact page, and your men took a copy of it.

Q. And that was in a late publication, wasn't it, and somebody's case?

A. It was a late publication, but no particular case. I have heard of it, but the date of this publication, I don't recall.

Q. Are you basing your testimony upon that kind of information, generally?

A. It is my general engineering knowledge.

Q. Do you consider that statement, that Dewar and Redwood got more gasoline than they wanted, general engineering knowledge?

A. Well, it happens to be my general engineering knowledge.

[fol. 643] Q. That question was whether you thought that Dewar and Redwood's disclosure was perfectly clear to the effect that they do not introduce gas, except in starting up?

A. I think it is, to one who is at all skilled in the art.

Q. And that it would not be necessary to do it in running their process?

A. It would not be necessary to do it, excepting at the start.

Q. Now, you are equally clear that the Dewar and Redwood process discloses heating the still continuously, and withdrawing oil from it continuously, are you?

A. I read from page 2, line 21:

"By another pipe, E-2, furnished with a cock, or valve, the contents of the retort may be drawn off, or this pipe may communicate with a second retort, which, in like manner, may be connected with a third, so as to form a series of any required number."

Also, the same words, essentially, are used in Redwood's Book on Petroleum, of 1896, page 322, Vol. 1, and if this

was done it would seem to me that the meaning would be that, as far as my individual still was concerned, it was essentially a continuous process, so far as ordinary cracking processes go—it can readily be a continuous process.

Q. There are two different angles to that answer, both of which I want to examine about. First, you say so far as any process of this kind can be continuous—now, how nearly continuously can you make a process of cracking distillation in a direct fired still, of this Dewar and Redwood type?

A. If we had several in series, the first one—that is, the one which would have the nearest to the feed oil, in it, would probably be able to be run for a considerable number of days—probably for several weeks, before it would be carboned up sufficiently to require hand cleaning. The last one of the series would have a much shorter life between cleanings.

Q. Can you consider such a process as a commercial cracking process?

A. I think that could be spoken of as a continuous still for alcohol, in the alcohol industry, and that only works five and a half or six days as a continuous still, so that I would say that when this works twice as long as that, it would be considered a continuous still.

[fol. 644] Q. And do you think that a process running in a continuous manner, such as you have stated, would be a practical, commercially successful process?

A. It has been, when working under atmospheric pressure, and I imagine that it would be when working under super-atmospheric pressure.

Q. Where has such a cracking process been operated at atmospheric pressure?

A. I didn't say a cracking process.

Q. I thought that was implied in my whole line of questioning.

A. It was a petroleum distillation process.

Q. But, has such a cracking process ever been run at atmospheric pressure?

A. I don't know of such a process being run at atmospheric pressure.

Q. And could it be successfully, practically and commercially run?

A. You could get your products out, and it would make products of good quality. Whether it would be a profit-making proposition, in the present-day process, or not, is a question.

Q. As a chemical engineer, reporting this to a client, would you say it was a practical process, or not?

A. I would want to spend considerable time, not two minutes, but several weeks' time in covering the details of it.

Q. Now, going to the Dewar and Redwood process—a continuous process, operating under pressure, that you have said that you find in this patent—is that, in your judgment as a chemical engineer, a commercially practicable and feasible process?

A. We were speaking, primarily, of the single distillation, but I am inclined to think that it would and might easily be, when used in a continuous form still. Whether it would be as commercially profitable as some of the present-day processes, I am not so certain.

Q. But you think it would be a commercially successful process to run a series, as you say, Dewar and Redwood say they meant to run, and——

Mr. Rauber: Your question seems to be indefinite. What do you mean—what condition do you mean?

Mr. Wiles: Let him fix his own condition.

A. You mean by commercial, that it would be a profitable [fol. 645] proposition—that it must carry a considerable percentage on the investment?

Q. Sure.

A. I will say yes, and I think I can answer your question this way—that it is as full a description, as complete a description of a process which would promise to pay profits, as any of the patents, or citations which are under discussion here.

Q. That doesn't answer the question, at all.

A. It doesn't answer it. I recognize that it only answer it in part.

The Master: Do you know whether any of the refineries are making a profit, or not?

The Witness: No, I do not, your Honor.

Q. Are you unable to answer the question?

A. What is the question?

(The Reporter read to the witness, the question, as follows: "Now, going to the Dewar and Redwood process—a continuous process, operating under pressure, that you said that you find in this patent—is that, in your judgment, as a chemical engineer, a commercially practicable and feasible process?")

The Master: The real question is whether it is an operative device, and, as applied to this, it is a question as to whether Dewar and Redwood's process would be operative to produce gas.

Mr. Wiles: Well, your Honor, there is another question——

The Master: Do you think Dewar and Redwood would be operative to produce gasoline?

The Witness: Yes, it would, without any uncertainty.

Q. Now, what is there in the Dewar and Redwood disclosure, that is inconsistent with the theory that they actually run without feeding in during distillation?

A. It is contrary to the general knowledge of such work. The purpose is to produce the pressure at the start, and it is known that you will get off gases, and——

Q. No, I mean feeding liquid during distillation?

A. Oh, I misunderstood you.

Q. What is there inconsistent with the belief that their process is one where, instead of feeding liquid during distillation, they charge their still, or, their series of stills, and run a certain amount without feeding, and move along and do it over again?

The Master: As I understand it, is there anything in their from which it is inferrable that the process was continuous, in the sense that the defendants' patents are continuous?

A. I understand that this is operable, either as a batch process or as a continuous process. As a batch process, from the statement on page 2, line 17, "E is a pump for forcing oil to be treated, by a pipe, E, into the retort, C, this pipe preferrably extending to the farther end of the retort. [fol. 646] By another pipe, E2, furnished with a cock, or valve, the contents of the retort may be drawn off."

That would mean, to my mind, that it was thought of, possibly, as a batch process.

Now, the next clause——

The Master: Now, let me ask you right there: If it was a batch process, would you have to force the feed into the retort?

The Witness: It might be a case where you would fill up your retort, operate for awhile, put in more material, but still not draw off your residue, and, in a sense, it would be a batch process.

Then, the second portion, beginning at line 21, which I read before—where it could be connected in series, and we could have a continuous process.

Q. It does not say continuous there—it simply says it could be connected in series?

A. So as to form a series of any required number.

Q. Now, when this particular patent was under consideration by the Board of Examiners in chief, in the United States Patent Office, they held, did they not, that the disclosure here was not clearly of a process in which there was any feeding during distillation?

The Master: Now, if you know anything about that, you may answer. If you don't know anything about it, say so. It is a matter of substantive proof in the defendant's case.

Mr. Wiles: It is in the Government's case, your Honor, they offered it here in exhibits. The decision is in evidence.

The Master: Did they offer a Patent Office decision in respect to this?

Mr. Wiles: Yes, your Honor, in respect to the Adams patent, in which this is referred to.

Mr. Rauber: It is in the file wrapper.

The Master: Well, the quickest way to dispose of it, if the witness hasn't read it, is for him to say that he hasn't read it, and he don't know.

The Witness: Well, I don't recall.

The Master: Then you have the benefit of it.

Q. Now, will you turn, Doctor, to the Chamberlain patent, please. Chamberlain gives, does he not, a rather complete description of the running of a coking still?

A. Would you mind giving me that page?

Q. Page 1, he starts it.

A. On page 1 he describes a coking operation.

[fol. 647] Q. Now, do you understand that this is a correct description of the ordinary cycle of operations in the or-

dinary coking still, as commonly conducted, for some years prior to Chamberlain's filing date?

A. I think that sentence, which begins on line 19, and ends on line 51, is probably fairly representative of the art of, say 1910-12.

Q. Now Chamberlain has particularly to do with the cuts higher than kerosene, hasn't he—in his patent?

A. Yes, for that purpose.

Q. We do not need to consider the kerosene cut, or anything lower than that.

A. You mean his raw materials for his cracking process?

Q. Yes.

A. Yes—partly, though, not exclusively.

Q. They are all the higher cuts—the cuts including kerosene, and lower, are not in here?

A. In some cases. As I read this, I am inclined to think that he includes even as broad a material as crude oil.

Q. Now, he first, then, takes off crude naphtha—that is his first cut?

A. Yes.

Q. Then he has a paraffin cut, which he marks B, water-white burning oil—that is another cut. Now he has a third cut, C, a mixed distillate, which consists, in part of hydrocarbons with boiling point normal for naphtha, in part of higher boiling hydrocarbons, with viscosities normal for burning oil, kerosene, and in part of hydrocarbons with viscosities too great for normal constituents of burning oil, kerosene, and not great enough to be normal in other than light grades of lubricating oils.

Is that cut marked C, what is commonly called the gas oil cut?

A. Yes, roughly, but it is, apparently, containing a larger proportion of products with boiling point normally for naphtha.

Q. It has got too much light ends, but roughly, and broadly, it is the gas oil cut?

A. Yes, sir.

Q. With a little more light ends in it than it ought to have?

A. I think that is correct.

Q. Now, the next cut, after that, is a paraffin distillate cut?

A. That is correct.

[fol. 648] Q. And the next one is slop, as he calls it?

A. A heavy, paraffin slop.

Q. Now, what is it that determines, in practical refinery operation, the end of the heavy paraffin cut, and the beginning of the slop cut?

A. I don't recall.

Q. Doesn't Chamberlain tell you very clearly, somewhere?

A. Possibly.

Q. I will call your attention to the first column, page 4—doesn't that tell you that the cut point between paraffin distillates and slop, is determined by the point at which the distillates will no longer press properly?

A. That is apparently the division point that he uses.

Q. Well, that is the normal, and would naturally be the normal point in refinery operations, to make the cut, wouldn't it?

A. Yes, but it is difficult to use it as a scheme of cutting. That is, you have got to use other methods when in practical operation. You can't take it out and take a sample and carry it to the paraffin plant and have it tested, and taking two or three hours in that operation and then come back and cut back at that particular point?

Q. Oh, no—I mean you find in normal operations, that when your distillate reaches a certain gravity it will no longer press well, and that is the cutting point?

A. Well, that is the gravity that I was looking for—or the temperature—and I didn't find it, and I don't think that this is really an answer to the question as to what point do you cut at?

Q. Your running orders to your still man would be based on gravity, ordinarily, because that is the only thing that he could observe quickly. But the reason for that gravity being selected is because that is the one at which it is found that you cannot press properly, isn't it?

A. Yes.

Q. So that the cut, including paraffin distillate, is the one that will press, and the slop cut is the part that will not press?

A. That is my understanding.

Q. And that is a definite dividing line that can be determined, and is, in fact, determined in all the refineries that run in this general way, isn't it?

A. I understand so.

Q. Now, the slop cut, itself, is taken and re-run accord-
[fol. 649] ing to this Chamberlain patent, is it not,—that is, re-distilled to coke?

A. Yes, that is my understanding of the second paragraph on page 4.

Q. And that slop cut is resolved into cuts corresponding to the original cut that came off of the crude?

A. Yes.

Q. And you get some more paraffin distillate, or material that is pressable, out of that operation?

Q. Now, Chamberlain's specifications are—and I am leaving out of the proposition any question as to how broad his language is, in defining it, but the actual material that he takes out of this old process, as his cracking stock, is the stock that comes out between kerosene and slop, isn't it, and added to it, the corresponding part that comes from the re-running of slop?

A. In parts of his patent, yes.

Q. Well, that is the only specific material that he describes taking—is the material from above the upper end of kerosene, and before the slop—is the only specific material that he describes taking as his cracking material.

Mr. Rauber: Is that a question?

Mr. Wiles: Yes.

Mr. Rauber: Well, the Doctor would have to read the entire specification, there, to find that out.

Mr. Wiles: Well, I want him to, if there is any question about it.

A. I find one place in disagreement with that, and I think there are others. I will give you this.

Page 6, line 122:

"While it is considered best to charge the pressure still with an oil which represents an entire range of hydrocarbon from and including some with the intermediate viscosities mentioned to and including those recovered from heavy paraffine slop, more or less of said hydrocarbons can be collected separately, if so preferred, and only the remainder of them charged into the still.'

That would be a case where he would be taking only the part above the gas oil.

Q. Where he would take part of that, instead of it all?

A. Yes, sir.

Q. That is true. There is no question about that.

A. I would say that the first paragraph on page 7, is also admissible, as expressed in one or more claims:

[fol. 650] "I have obtained larger yields, however, both of naphtha and of burning oil—kerosene—from distilling mixtures of hydrocarbons with viscosities normal to heavy lubricating oils, and hydrocarbons with the intermediate viscosities mentioned than from distilling like volumes of said two kinds of liquid hydrocarbons separately; and I believe that the same principle would hold true in respect to mixtures containing hydrocarbons with viscosities normal to medium lubricating oils. It is also my belief that better results are attainable by distilling oil in which two or more of said three kinds of liquid hydrocarbons are naturally present than by separating such hydrocarbons and then forming a mixture artificially of them in their original proportion."

It is hard to read this sentence, and the meaning is so indefinite that one is not certain what is meant.

Q. He is not specific as to any mixture, at all?

A. I am not certain regarding the meaning of it, but it is certainly broader than a straight distillate, carrying the petroleum which had been earlier mentioned.

Q. If it was made from the same crude oil it would all come out of these same identical fractions, would it not?

A. But, as I understand it, this is a case where he is allowing, here, an artificial mixture, which may not be from the same crude oil.

Q. If it came out of a different crude oil it would be the same corresponding cut, and——

A. But if it came from a shale oil it wouldn't come from a crude oil at all.

Q. I am not worried about that—it would be the same corresponding cuts between kerosene on the one hand, and slop on the other, wouldn't it?

A. But the viscosity would be different, and this is a condition which is dependent upon viscosities.

Q. You think that might broaden the specific illustration which he gives?

A. Yes.

Q. All right. Where else?

A. In the succeeding paragraph he also says:

"Such heavy paraffin distillate in each of said forms contains liquid hydrocarbons with viscosities normal to heavy and medium grades of lubricating oil, and also hydrocarbons with the intermediate viscosities mentioned in such relative proportions that in the distillation under pressure, the decomposition of the hydrocarbons of each of said kind is modified by the presence of the two other kinds."

[fol. 651] "For exerting such modifying effect the hydrocarbons of any of such kinds should not be less than about ten per cent, by volume, of the sum total of the three kinds, and in the distillate mentioned, the kind present in least proportion is in excess of such ten per cent. Said distillate is also so far free from still wax, and like heavy matters of sticky characters and from elements yielding the same or coke that charges of the said distillate of practical size can be distilled under a super-atmospheric pressure of 75 pounds to the square inch to the extent of fifty per cent or more by volume without heating the still bottom to redness."

Now, if it contained the paraffin wax, it would not have the viscosities which he mentioned earlier.

Q. But that is a part of the paraffin cut, isn't it?

A. Yes.

Q. I asked you whether he didn't have the cuts between kerosene—between the upper end of the kerosene, and the beginning of the slop cut. Now, paraffin wax is a part of that cut, isn't it?

A. Yes, but I understood that you were referring, here, to the supposition that the paraffin had been removed.

Q. Oh, no.

A. Well, then, if you are not, this would be no answer to your original question.

Q. He might have put in a little more paraffin than was originally there, but there would have been some in that cut?

A. Yes. *A.* Yes. On page 8, line 43, he removes largely the limitation which he had placed earlier, in the absence of still wax, that is, from the heavy slop and says, "that it can be used as well, if it is not sufficient in amount for an

externally heated still of the dimensions hereinbefore mentioned for still, A, charged 6/10ths under a super-atmospheric pressure of 75 pounds per square inch at the aforesaid rate, 2 per cent per hour to the extent of 50 per cent or more by volume of the component hydrocarbons which have viscosities above 70 seconds at 100 degrees F., without heating the still bottom to redness."

That limitation, of course—by defining that the oil has still wax to that extent, that removes a good deal of the limitation present earlier.

Q. Now, having in mind the specific material which he prefers to use, that is, that cut from the upper end of kerosene to the end of the paraffin distillate, endpoint being determined by the point at which still wax comes over in [fol. 652] sufficient quantity to interfere with pressure; please tell me where in the art you find that stock for a cracking stock, alone and by itself without something else. I do not mean crude oil which has that and some more, I mean the stock from which the slop and everything higher is eliminated.

I will limit my question further, Doctor, to cracking stock in a pressure distillation process.

A. I have such references for atmospheric distillation cracking schemes but for pressure distillation I do not, at this instant, recall any that is exclusively this material. There are a number of which use the residue with the same lower boiling cut point, but do not use the same upper cut point as you suggest. There are those which particularly include the broader material, as given in his specification on page 8, lines 43 to 59.

Q. Those references which you have in mind are reduced crude containing everything above kerosene, are they not?

A. Essentially so.

Q. Yes.

A. Though sometimes they are distillates which have been used in a pressure cracking process or a gas oil and then re-used, and such re-used products would come very close to the specifications of Chamberlain.

Q. They would not contain the lubricating and wax fraction that he puts in?

A. They would not contain any large amount of paraffin wax; they would contain oils which could be used as lubricating oils. Will you care to have the citation?

Q. And would those oils be of the same character as the oils constituting the paraffin distillate cut from crude?

A. Essentially so, excepting for the paraffin.

Q. The same series, same gravity, viscosity relations and so forth?

A. No, I think they would be probably different in proportions and probably somewhat lower in viscosity. It depends somewhat, though, on what original oil you start with. If Chamberlain started with some of the character of some crude oils, it would be very close to those residues.

Q. Now, you cited the use of this particular cut in atmospheric cracking; where do you find it?

A. It has been read into the evidence once.

Q. Just tell me the name. I did not remember that you had.

A. I will have to spend a few minutes looking it up here. [fol. 653] Q. There are some things in this record I can not quite remember.

A. I think I have the reference now: From Crew, "A practical treatise on petroleum" of 1887, reading from page 280 after he has been discussing the cracking of petroleum to make illumimating oil:

"After all the oil that can be safely run into the illuminating oil has been removed from the still and when the darkened product would impair or imperil the color test, the fires are usually withdrawn from the still and preparations made to remove the residium and the coke. Some refiners prefer to continue the distillation for a short time longer to recover a small percentage of oil, dark though it may be, which is run over with a fresh portion of crude oil."

That would be a portion that would be rather relatively light.

Now, in speaking of the treating of the residium, under the heading of "Treating of Residium":

"This is run down in a coking still," and he speaks of it further on page 283:

"When the still is loaded and the fire started, the distillation commences. The first running from 20 to 25 per cent (if the residium be of good gravity) say of 20 degrees to 21

degres B, will be found sufficiently light in gravity to be transferred to the crude oil tank and re-distilled from this."

Now, a 20 to 21 degree Baumé would be an oil of the density of our heavy paraffin distillate, probably. It would at least contain the paraffin but might be the oil portion of it. This would be typical, in general, of a lubricating oil group but it would not have paraffin, probably, in any considerable amount.

Q. Now, Doctor, in both of these cases this distillate that you speak of was put back into the crude oil, was it not?

A. To be used as cracking.

Q. But it went back and was mixed with crude oil, was it not, containing the entire crude?

A. Yes, that is my understanding.

Q. So that when the oil went into the cracking still it contained the slop and the still wax and the coke ingredients, did it not?

A. Yes, some.

Q. Now, my question was addressed to where you found the Chamberlain stock by itself, not with something else, but that cut which excluded the ends beyond the paraffin distillate from your cracking stock? You do not find that [fol. 654] anywhere, do you, either in pressure or atmospheric distillation?

A. I have not before looked for it particularly, and using that definition of Chamberlain stock, which is not his definition, not the definition in the patent, then I think you are correct, that I have not found it. It may be present but I do not at this time know enough of it to speak of it.

Q. Now, when you say that this is not the Chamberlain definition and that Chamberlain has broadened his definition, you have in mind the paragraph beginning on line 43, page 8, do you not?

A. I have.

Q. And it is your contention, or your position rather, that that paragraph broadens the description to such an extent as to include ordinary crude residiums, reduced crudes?

A. Yes, many of them.

Q. Dr. McKee, at the close of the testimony on Friday, there remained on the record an unanswered question.

My question was—on page 2077, of the typewritten transcript:

"And it is your contention, or your position, rather, that the paragraph"—meaning the paragraph on page 8 of the Chamberlain patent, as to the test which was then under discussion—"broadens the desscription to such an extent as to include ordinary crude residuums and residues?" [fol. 655] And your answer was, "yes, many of them," and I then asked you, "For instance?" meaning to ask you to give us some examples of the crude residuums that would, in your judgment, respond to that test in the Chamberlain patent. Will you please answer that, now?

A. I would think that a residuums left by removing the gasoline and kerosene from Pennsylvania, or Mid-Continent—or, many of the Mid-Continent oils—would be ones which would correspond to this.

Q. When you say "many of the Mid-Continents," do you mean the average run of Mid-Continent oil?

A. No. I am not thinking, of the exceptional ones—the ones, for example, which carry practically no light ends, and, which, consequently, could not be topped, and are extremely heavy in asphalt materials. But I am thinking—as there are several fields in the Mid-Continent—for example, of the Coffeyville Field, of Kansas, of a few years ago, and also of those oils which are average oils, rather than those which are extremely heavy in heavy constituents.

Q. You mean the average ones would respond to the test—the residuums from the average oils from the Mid-Continent fields?

A. I am really thinking of individual oils, rather than of a mixture of oils from there, and I am thinking that some of those extremely heavy oils which are found in exceptional cases, in there, would, perhaps, not do it, but that if we took all of these samples from the various fields, there, that probably the average oil of the Mid-Continent would respond to it, and I gave, as an example, or had in mind, such as this oil at Coffeyville, Kansas.

Q. Now, referring to the Palmer patent on page 530 of the printed record, you stated, in answer to a question by the Master, that in this patent the implication of the specification and the claims was that the digestion and distillation contemplated by Palmer, were separated in time—that is, as I understood your position, it was that Palmer sets forth digestion without simultaneous distillation. Is that correct?

A. That is my understanding.

Q. Isn't it a fact that in each of the references that you cited against Palmer, to-wit, Thorpe and Young, Engler, Benton, Laing, and Barbet, the distillation and digestion are simultaneous?

A. I also cited Lobry DeBruyn, on page 526 of the printed record.

[fol. 656] Q. I must have overlooked that. I will add that to the question.

A. Lobry De Bruyn, heats in a sealed, closed glass tube—

The Master: I think that question ought to be answered distinctly as to whether those patents do show simultaneous heating and decomposition and reaction, or whether they show a period of time between heating and the reaction, or digestion.

A. (continued): Benton, in part, describes his process as a process in which there is separate digestion, and then follows this with the liberation of it—that is, reducing the pressure. I refer, particularly, to page 1, line 76:

"My invention consists, essentially, in subjecting the crude petroleum to a high heat, and pressure in the chamber, or pipe, or vessel, and so regulating the heat applied as to raise the temperature of the crude liquid to about 700 degrees Fahrenheit, or higher, while maintaining a pressure of 500 pounds per square inch, more or less. Under this pressure the liberation of any vapor from the highly heated crude liquid is prevented until the heat has been continued long enough and raised high enough to bring the entire body of the oil confined in the chamber, or pipe, or vessel, to a uniform temperature, when it is "next" "discharged in a spray, in the vapor chamber, in which the previous high temperature is still maintained, but the pressure removed."

Q. That is Benton, isn't it?

A. Yes, sir.

Q. I thought you were testifying about Lobry De Bruyn, first.

A. I canceled that and started in with Benton.

The Master: I called him back from that, and told him to take up all of them.

The Witness: In the case of Lobry De Bruyn, we have a somewhat similar digestion at high pressure—a pressure of approximately, as Lobry De Bruyn states, 15 atmospheres.

Q. In that case, isn't the distillation a part of the process, going right along with it? Isn't that a pressure distillation process?

A. Only incidentally—not primarily.

[fol. 657] Q. I want to ask you, now, in the Lobry De Bruyn, the stock is put into a pressure still—into a Riebeck still, isn't it?

A. No. It is put into a glass tube. It is a lecture experiment.

The Master: This is laboratory. Isn't this the effect of it—that Palmer says, "There must be some time and some place where you permit, after having decomposed the molecular structure of this oil, the necessary reactions to form the new products—to synthetize the products which you desire to obtain?

Mr. Wiles: And without simultaneous distillation.

The Master: And he says the time may be very short. The thing that he emphasizes is permitting the reaction to take place under pressure and temperature.

Mr. Wiles: Quite so.

Q. Now, in the Benton process, you say the steps are separated in time?

A. They may be—on the one, there, it is described and [fol. 658] apparently there is an alternative one, in which it could be made continuous—in Benton.

Q. Now, in Lobry De Bruyn, the stock is fish oil and water, isn't it?

A. Fish oil, I think, alone.

Q. And in Lobry De Bruyn, you used a bent tube with two ends, don't you?

A. A bent glass tube.

Q. One end of which is a still, and the other is a condenser, isn't it?

A. Only incidentally, however.

Q. One is cold and the other is hot, isn't it?

A. Not particularly cold. One is cooler than the other.

Q. And only one end is fired?

A. That is correct.

Q. And from the fired end, in which you put your fish oil, the vapors rise and go over and condense in the other end?

A. Yes, sir.

Q. So that it is cold enough to condense?

A. Under a pressure of 200 to 400 pounds.

Q. And the light ends formed, do not stay in a liquid body, in the hot end, but go over into the cold end and boil off just as fast as they are formed?

A. So I understand.

Q. So that, to that extent the process is precisely the same as in an ordinary pressure still, with a condenser—the material boils out of one end of the system and condenses in the other?

A. There is some analogy—there isn't a complete analogy, of course.

Q. And that is also true of Thorpe and Young—they use either a bent glass tube, or two mercury bottles, and boil the stuff out of one end, which is hot, and condense in the other end, which is relatively cool?

A. That is my understanding. I don't think, Mr. Wiles, that I gave Thorpe and Young as a reference there. Possibly I did.

Q. I think you did. At least, I have it in my notes, and I don't know how I got it if I didn't get it from your testimony.

In Engler the same thing is true, except that the pressure does not extend over into the place where the condensing is done. That is, Engler is talking about Riebeck's [fol. 659] apparatus—Engler had a Riebeck apparatus set up, with fish oil, and had it run for him, and he talks about the product thus obtained—I mean the Engler one that you cited. That is found on page 526 and 527, of the printed record.

A. I presume this is the Engler of 1893—Journal of American Chemical Society—and if that is correct, your statement is correct.

Q. Didn't he use the fish oil apparatus with the fish oil charge?

A. He doesn't mention Riebeck, but I think it is.

Q. The other publications show that that was made by Riebeck for Engler?

A. He did use it, but whether he used other things he doesn't say.

Q. And in that process the fish oil would be distilled, so that just as fast as the light products formed, they would boil out and go out through the valve and be condensed?

A. Probably.

Q. Now, in Laing—Scotch Laing—the same thing is true—that is, that is distillation in the ordinary pressure still, and as fast as light products are formed, they boil out?

A. That is correct, excepting, of course, that there is digestion until the pressure of the valve limit is reached.

Q. And whether that will be long enough to make any difference in the constitution, you cannot tell by reading Laing?

A. Undoubtedly there is some cracking taking place during that time—if that is what you mean. It is not simply incidental—I mean, it is a fact that is of some importance.

Q. That would necessarily depend on the speed of heating, would it not?

A. Yes.

Q. And you can take a device like Laing's, and heat it very rapidly and get it to distilling before there is any substantial change in the material in the charge at all, can't you?

A. Laing's apparatus, holding perhaps 2,000 gallons, is not one that you could heat in a minute. It will take some real time to heat that up to a temperature of, roughly, 400 degrees Centigrade.

Q. Where does Laing disclose his temperature of 400 degrees Centigrade?

A. In the corresponding—I am mistaken about him disclosing a temperature of 400 degrees Centigrade. He discloses a pressure of 480 pounds.

[fol. 660] Q. Yes. Now, whether there is, or is not cracking sufficient to modify materially the character of the stock, during the period that has been spoken of—that is, before distillation commences—depends entirely on the rapidity of heating, does it not?

A. Yes, and the kind of apparatus that it is in.

Q. Now, you have spoken of the capacity of the apparatus, as disclosed—where do you find that?

A. In an article in the Transactions of the Royal Scottish Society of Arts, of 1894, in an illustration, figure 2, and with it is a scale which permits us to calculate the length and the diameter. Such a still would hold, roughly, 2000 gallons—that is, it would be twice the capacity of the steel street sprinkling wagons on the New York City streets.

Q. Now, from the scale what do you make out the dimensions of the Laing still to be?

A. A little over 15 feet long and a little over 4 feet in diameter—say 4 feet, 3 inches, in diameter, and about 15 feet, 3 inches, long.

Q. How long?

A. No, it is longer than that, I beg your pardon.

Q. It is about sixteen feet six, isn't it?

A. Yes—something like that. It is 17 feet—4 feet, 3 inches, by 17 feet.

Q. It is almost exactly 4 feet in diameter?

A. I get it 4 feet three.

The Master: Well, that is near enough for the calculation, on a drawing of that kind.

Q. Then, the total capacity of that still, as illustrated, if filled brimful, would be eighteen hundred gallons, approximately, would it not? I have a table of cylindrical tanks, here, if you want to look at it.

The Master: What difference does it make, whether it is eighteen hundred or two thousand gallons?

The Witness: Well, I made a rough calculation, before, and it came to two thousand.

Q. And that still would be charged with the outage, which is shown in the drawing, would it not?

A. The charge would be that, yes.

Q. That is, only about two-thirds of its vertical height would be filled with oil?

[fol. 661] A. It would be about, between two-thirds and three-fourths.

Q. And your position, then, is that this preliminary heating of Laing, must necessarily produce a substantial change in the character of the oil, before distillation commences?

A. Some change—depending upon the rapidity of the heating, and how full the tank is, and the character of the

oil—that is how quickly that particular oil changes in character.

Q. My question was, whether it would be substantial—would there be any substantial change in the boiling point, of necessity, in the Laing process, before distillation began?

A. Yes, I think there would be.

Q. How much?

A. As I said a moment ago, it would depend upon the oil used. It would depend upon the fuel used—the fuel, apparently, used here, is coal, judging from the kind of grate bars indicated—so that the speed of heating would not be extremely rapid, and it would depend upon the amount of oil—that is, whether it was filled just to the point indicated here, or less, or more. The larger variable factor is the character of the oil used, some oils cracking easier than others.

Q. Now, in Barbet the same condition applies that applies in Laing, does it not?

A. Will you tell me which Barbet that is?

Q. Well, the Barbet you referred to?

A. I have no note here, and I could find it easier if you gave me what I referred to there.

Q. Well, you referred to No. 322,265.

A. Answering the question, I will say essentially so.

Q. In the course of your long answer to Mr. Neave, where you summarized your views on the whole group of patents, you included the Burton safety valve patent, without stating your reference to it. Was your reference Atwood, which was offered by the Government at the first hearing in this case?

Mr. Rauber: Are you sure he mentioned that?

Mr. Wiles: Oh, yes, he mentioned that patent, but he didn't give his reference.

Mr. Rauber: Whereabouts is that?

Mr. Wiles: It was early in Mr. Neave's cross examination.

A. Yes, and Gaster, British patent, No. 6739, of 1901. I [fol. 662] have those two in my memorandum. I do not, at this instant, recall what I had in mind when replying to Mr. Neave.

Both of these describe safety valves connected to a pipe to carry away vapors from the part of the building where they might cause fire, or explosion.

Q. Now, having reference, first, to Atwood—Atwood is an atmospheric pressure still, isn't it?

A. It happens that I do not have a copy of the Atwood patent here. I did have it, but I haven't it now.

Q. I will give you one.

A. Answering the question, I will say that it is primarily so. He cracks just above atmospheric pressure, as I understand it.

Q. What do you mean by just above—just enough to let the vapors go out? There is no valve, or control on it, is there?

A. No, excepting that we are passing through, here, a vessel, which will give us some back pressure.

Q. A few ounces, or maybe a half a pound, or something like that?

A. Probably more than that, with as many plates as he indicates—perhaps three pounds.

Q. Now, the ordinary atmospheric still has its safety valve set below five pounds, has it not, in the practical art?

A. It is customary to set a safety valve at a few pounds more than the operating pressure; say, in ordinary work we will set it from three to ten pounds below the normal operating pressure.

Q. You can set it anywhere, but what is the fact, Doctor? Is it not the ordinary refining practice to set it at under five pounds?

A. I do not recall any ordinary refining process at the present day working at approximately atmospheric pressure but a few pounds above it, with a back pressure such as this. Most of them will work practically without back pressure.

Q. They all have some sort of a tower on them now-a-days, have they not?

A. No, I do not think they all have.

Q. Does not the ordinary standard still which is used in common American practice, and has been used in common American practice for 18 or 20 years, have some sort of a tower on it always, or practically always in the great majority of cases?

A. If you will cut out the "always"——

[fol. 663] Q. In the great majority of cases.

A. In many cases it does, and in such a case we would probably set it at five pounds more than the operating pressure.

Q. What I am getting at is this: Is it not a fact that in ordinary refinery practice the safety valve is set below five pounds?

A. I do not know on that. I would rather doubt it. I would expect it to be set between five and ten.

Q. You think two pounds would be unusually and improperly low?

A. You would be liable to have leakages out that would be troublesome.

Q. You think the normal practice is to set it higher than two pounds?

A. The normal practice would be to set it at higher than the particular pressure——

Q. Oh, of course it would, but I am talking about normal practice in pounds in a normal, ordinary refinery; how do they set the valve?

A. I told you that I did not know.

Q. Oh. Now, in Atwood, the safety valve is stated according to the specification to blow when the condenser is clogged, is it not? That is about the middle of the first column on page 2, speaking of the safety valve, it says that, "through which vapors can escape when from any cause the worm becomes choked."

A. That is correct.

Q. Now, the only cause that anyone could normally anticipate for choking a worm in running heavy, paraffin-like products, is the clogging of the worm with paraffin, is it not?

A. That would be the most likely cause.

Q. And in that case the vapors that came off would be of a very high boiling point and condense very easily?

A. Yes. It must be borne in mind, however, that this is primarily for oils from coal rather than from high paraffin oils of the petroleum character.

Q. Those are very high in paraffin, are they not; in wax?

A. Some of them are.

Q. And when the wax is coming over, that is the time when you would expect your worm to choke up, that is correct, is it not?

A. That might easily be.

Q. Now, the flow of gases and vapors from a safety valve [fol. 664] will increase very rapidly as the pressure within the still rises, will it not? That is, it will vary in direct proportion to the internal pressure in the still at the moment the valve opens?

A. For a given length of time for the valve to be open, yes.

Q. The rate of exit will vary directly with the pressure, or something like that?

A. Yes.

Q. Or with the pressure difference on the two sides. And in the case of a Burton stil-, running at say 75 pounds, the eflux of vapors when the safety valve blows will be very much larger than it will from an atmospheric pressure still, will it not?

A. Yes, if this opening is of the same size. It must be borne in mind, however, that with increase of pressure we always either increase by a spring, or its equivalent a weight so that the opening ordinarily does not open as far for a high pressure still as it does for a low pressure still.

Q. No, in a pressure still running, say, at 75 pounds, the stock in the still is very greatly superheated, is it not, so that if the pressure is released there will be, of course, increased evolution of vapor?

A. Yes, if the pressure would be released say at atmospheric pressure, or anything of that kind; but if it was released down only to a certain point it would be a very minor amount of vapors.

Q. It will be very large if there were any pressure reduction?

A. If you, by your safety valve, open it long enuogh to actually reduce the pressure within the still a decided amount.

Q. And in an atmospheric still the pressure for superheating would not be so great, so that the release of a relatively small amount of vapors will cool the contents of the still enough so that boiling stops?

A. That is correct. The amount of reduction pressure or the amount which it will release will depend upon the amount of reduction of pressure within the still, and if you reduce it in one as much as in the other——

Q. But you can not reduce it as much in one as in the other, can you?

A. Yes. That is, in an atmospheric still working with safety valve set at say eight pounds, you could reduce one [fol. 665] or two pounds pressure; and with one set at 80 pounds you could reduce one or two pounds pressure.

Q. Now, in the Atwood patent the vapors which come out of the still with the safety valve closed, go into an auxiliary condenser and they are supposed to be condensed and saved, are they not?

A. A part of it, for it says, "the condensable products escaping through the safety valve and saved in the main, J, flow through at the small pipe, J^2, into any suitable receiver; the fixed gases and vapors not condensed escaping through the discharge pipe, J^3.'

In other words, they are a part of the vapors that are not condensed and a part, possibly, that may be condensed.

Q. And the theory is that the bulk of those vapors are condensed; the essential things which get away are fixed gases and very light vapors that would not condense anyhow?

A. I think not. There is no statement here that they would not condense if they were given more time, but that they are part of the vapors, those that are readily condensed or condensed and those which have time enough to condense do escape and are carried off through the roof of the building, or in any suitable manner, away from the vicinity of the still, he states.

Q. Does it not appear that the prime object of this arrangement is the conserving of the distillate which would otherwise be lost?

A. No, as I understand it, it is a question of carrying it away from the vicinity of the still so that there can be no danger from it. He would not call it a safety valve if it was simply a condensing scheme for recovery. I think the prime object of the safety valve is for safety.

Q. It would operate for the recovery of your condensed products, would it not?

A. Yes, but the condensed is only incidental, the possibility of recovering some materials which would otherwise go to waste and would make still greater danger.

Q. He does condense a considerable part, does he not?

A. I would think not, for the pipe, J, which he speaks of as catching this product and condensing it, simply an air exposed pipe; that is, not water cooled or anything of that kind; and the pipe, J^2, which he says, "may flow through the pipe, J^2 into any suitable receiver" looks as if he expected from time to time to take it off in a bucket; as if it was a very minor amount.

[fol. 666] Q. Now, what he says is that the condensable products escaping through the safety valve and so on are saved, does he not?

A. Yes, those which are condensed by such air-cooling condenser, and the fixed gases and vapors not condensed are passed——

Q. He says, "condensable" not "condensed," does he not?

A. Yes.

Q. So that a mechanic trying to follow this process, would build a condenser that would condense the bulk of the products that might come out of the valve, the bulk of the condensable products?

A. He would probably attempt to build it like the drawing, which is simply an air-cooled pipe there.

Q. You know that the proportions in drawings are merely digramatic, don't you?

A. To a certain extent.

Q. Yes.

A. But they are——

Q. And when the specification says "to condense the condensable products" would not the mechanic skilled in the art, trying to follow that disclosure, build a device which would condense at least the bulk of the condensable products?

A. I think your air scheme would condense the bulk of the condensable products in that it would only blow off occasionally, and the pipes, meantime, would be cooled down to somewhere near air temperature.

Q. That is, they would be cool enough for the little flow that occurred in an atmospheric still, and would actually condense the bulk of the condensable products?

A. Condense the bulk of them but not all of them, probably.

Q. Now, I wish you would sit down and calculate for me, please, as a chemical engineer, how big you would have to

make an emergency condenser to condense the bulk of the condensable products escaping from a pressure still at 75 pounds, running, on a gas oil charge when an ordinary safety valve blew; just calculate for me how big it would have to be, assuming that the valve released in a normal way, and assuming, too, that the still is of 8 by 30 size and charged 5/6ths full, or perhaps you can answer it this way: Is it not apparent on inspection (and this would be satisfactory) that the condenser, emergency condenser would [fol. 667] have to be much larger than the normal, regular condenser and bigger than the still?

A. With the safety valve serving, as it does, for only time to time action, as I understand your question, and the reduction in pressure say from an 80-pound to a small amount, such as a good working safety valve would give, that condenser would not need to be, if properly made, anywhere near as big as the still itself.

Q. Now, with reference to the Laing paper, you have several times referred to the alternative method of condensing under pressure. Set forth toward the end of the paper. The fact is, is it not, that Laing, while he mentions that method of condensation, states that it is undesirable and discards it.

A. He states that it is difficult, as he states on page 159, "Your fittings being under pressure could not be kept tight through the expanding and contracting action which was always at work."

And further, "That the product, as it comes away from the condenser is 'surcharged with gas,' like an aerated water when the cork is drawn, and that meant loss of material as well as risk from fire."

He, is of course primarily interested in kerosene rather than in gasoline, and as such the question of retaining the light ends is of little importance to him.

He discusses both methods, and then gives a scheme which he feels gets away from these difficulties in a relatively, as he calls it, "simple way," but prefers for this kerosene work, in which gasoline is only made incidentally, to use it without condensing under pressure.

Q. Now, he refers to this method of condensing under pressure as if it were a prior art method, does he not, and a known method before his day, and claims two methods have been employed to do this?

A. Yes, it is old.

Q. Now, when he refers to the pressure condensation method, what art do you understand he was referring to? I can even make this more specific, doctor: Do you understand that it was Dewar and Redwood who were about contemporaneous with him or some other art that has not appeared here?

A. I have a tabulation which is a chronological tabulation [fol. 668] that may help me in this. I cannot tell from the context there just to what methods he was referring. This was printed in 1894, but delivered as a lecture in December, 1891. We have as prior to that his own apparatus which was said to work as under 80 pounds pressure, possibly Pielsticker's, possibly Schuchow, possibly Intschick—

Q. You have got me wrong. I meant which of those had pressure in the condenser, through the condenser, that were prior to Laing?

A. I do not know to what he referred; it might have been to the Dewar and Redwood, though we have still others which possibly might.

Q. I am interested in what anyone besides Dewar and Redwood condensed under pressure, Doctor, if anyone does.

A. We were discussing in the Lobry *d* De *d*ruin—no, that was the next year, that was after.

Q. Yes. Thorpe and Young?

A. Yes.

Q. Then a Thorpe and Young liquid does not come away at all in this charged fashion, it all goes over and condenses down in this mercury bottle, does it not; it is not drawn off during distillation at all?

A. Of the citations we have had here——

The Master: Laing himself does not say what pressure condensation he referred to; is it not all speculation?

The Witness: He simply says——

The Master: It is all speculation as to what he had in mind.

The Witness (continued): "Two methods have been employed."

The Master: That is all there is to it. He may have known of prior arts that does not appear on the record at all.

Mr. Wiles: That is what I was trying to find out. I have never found anything except Dewar and Redwood who were very closely contemporaneous.

Q. Now, you have told us and said a number of times, Doctor, that Laing was primarily interested in kerosene. You gather that, I presume, from his paper.

A. I think that was true in general of those people who were doing cracking at that date, that gasoline cracking was of no interest—cracking primarily for gasoline was of no interest until very much later than 1891.

Q. And the early part of the Laing paper seems to indicate that, too, does it not?

[fol. 669] A. I do not recall regarding that but I would expect that that would be true.

Q. In the third paragraph of the paper, is there not a pretty plain indication that kerosene is the thing that he is interested in when he refers to Russia?

A. Yes. He speaks of there being a large market for such a product.

Q. And that, you think, really is the thing that correctly states his true aim and object, to manufacture kerosene?

A. Primarily, but not the exclusive product.

Q. Now, I wish you would give us your theory, Doctor, to explain why, if that is true, as I think it is, he gives you, in the latter part of his paper and in his patent also, only one specific example, and that specific example is making a 70 degree distillate.

A. It is impossible, of course, for me to go back and say what was in the man's mind. I presume that his product, which he quotes there as giving a naphtha as a result of the product, was simply one of the products which he ran and which he felt that he ought to give as an illustration in his patent. It may not have been what he mostly desired, but it showed that he got gasoline from his product.

Q. That is, solely gasoline; that is not a mixture containing some gasoline; that is, straight 70 degrees distillate coming off the still, is it not? In the paper I called your attention to that.

A. I do not remember, except I remember it as a naphtha. I will look it up.

Q. In the paper he gives the temperature at which he has to hold the last compartment in order to get that kind of a distillate?

A. He states that "a pressure of 80 pounds per square inch will be found suitable pressure for operating on a mineral oil of .970 specific gravity in order to obtain therefrom an oil of about .700 specific gravity." That would be a thorough cracking process that will give a gasoline product.

As I say, I cannot go back and tell what was in his mind and why he chose that.

Q. And the temperature he gives in his paper, in his specific example, is also aimed at that particular product, is it not?

A. You are referring to it as if he gave this product in his Long paper. I am looking for it; I do not find it. I do not find any temperature given in the United States patent, [fol. 670] which I have just quoted from. Possibly you can call my attention to where that product is given in the long paper.

Q. On page 157 of the paper, Doctor, a little below the middle of the page.

A. Page what?

Q. 157. He says, for instance, if you wish an oil whose boiling point is 250 Fahrenheit, you must have you- last departure kept steady at that temperature. Now, that also has reference to this same general type of oil, about 70 gravity, it it not?

A. That would be in that same general range.

Q. Now, referring to the Burton re-run patent, No. 1,105,961, that patent includes, does it not, three separate steps in its process:

First, the distillation of the original stock under pressure; second, the distillation of the residuum left in the still at the end of the pressure distillation operation at atmospheric pressure; and, third, the distillation under pressure of the distillate obtained as a result of the second step?

A. Yes, that is the preferred procedure.

Q. Now, which single one of the references which you have cited against this patent disclose those three steps, and all of them?

A. Noad does the same, excepting he does not distill under pressure—crack under pressure; that is, the heavy products.

Q. In other words, Noad lacks the first and third steps, is that what you mean?

A. No, I do not mean that. He cracks, he takes the heavy products that distill off at atmospheric pressure and then he recracks them, but he does not recrack under pressure.

Q. Burton takes the material that does not distill off under pressure, does he not?

A. And he also takes the residue from the rectifying still and recracks it, Noad does.

Q. Now, what I am trying to get at is, you say that Noad has——

A. (continued). Then——

Q. Just let me get this thing on Noad clear, first. Burton cracks first under pressure—distills under pressure; does Noad do that?

A. No. I say, he does not crack under pressure.

Q. He does not do Burton's first step, then?

Mr. Rauber: You said he cracked, did you not, Doctor? [fol. 671] The Witness: Yes, he cracks.

Q. He cracks, but not under pressure?

A. Yes.

Q. Cracks at atmospheric pressure?

A. The difference between Noad and the other is simply the adding of pressure to it.

Q. Now, let me get just what you say Noad does. You say Noad first cracks at atmospheric pressure, is that right?

A. That is right.

Q. And does Noad have a residue from that residuum that has not been distilled off?

A. He has part residuum that has distilled off, and part that has not; part residue that has been distilled off at atmospheric pressure. He suggests the use of both in the re-cracking process.

Q. Now, I am interested for this purpose only in the part which does not distill off under pressure, as does Noad, and part which does not distill under pressure.

A. He has not used pressure, as I said, at any time.

Q. Well, that which does not distill in the first cracking operation; has Noan a fraction which is not distilled in the first step of his process?

A. He has a residue, yes.

Q. That has not been distilled?

A. Yes.

Q. That is left behind as a residuum or tar?

A. Yes.

Q. In the cracking process?

A. Yes.

Q. All right. Now, take that residuum which Noad does not distill off, but remains as a tar at the end of this process; does he distill that at atmospheric pressure?

A. He has distilled a part of that residuum—he has combined two steps of Burton so that he has cracked and he has distilled at atmospheric pressure products which he then re-cracks, which is what Burton does, excepting Burton adds pressure to it.

Q. Now, Burton has a tar at the end of his first step, has he not?

A. Yes.

Q. And Noad has, too, according to your statement?

A. He has two,—Noad has two things, he has some tar he has not distilled and some that he has, and he cracks both.

[fol. 672] Q. The Burton process is interested in the tar that he has not distilled?

The Master: Both.

Mr. Wiles: No, just the part he has not distilled is all this patent has to do with.

Q. Now, Burton and Noad both have an undistilled tar at the end of this first step, according to you, and in addition, Noad has a distilled product; so has Burton, has he not?

A. No. I am referring to a distilled heavy product, a distilled residue.

Q. All right. But I am interested only in the tar that is left behind and not distilled.

A. You are interested in the part that does not?

Q. That does not distill in the first step.

A. (continued). Disclose Burton's process, then?

Q. What?

A. You are interested in the part that does not disclose the Burton process, then?

Q. No, I am interested in the corresponding steps. Burton has a tar left at the end of his distillation, of his first distillation, and you say Noad has, too?

A. Yes, but that is the part that does not disclose the Burton process.

Q. All right. Then Noad does not take the undistilled tar in the first step and re-distill at atmospheric pressure, which Burton does?

A. Not as a separate step, no.

Q. And he does not take the distillate from redistilling the tar and put it back in for another step?

A. Yes, he does. Noad takes and distills at atmospheric pressure and get- over two products, and which is the cracked products he wants, and one a residue. Now, that residue would be the equivalent; or would be essentially the same as a distillate from the residue, such as Burton gets. That product is then re-cracked, so that the difference between Burton and Noad is the use of pressure only during the cracking process.

Q. Now, Noad has a tar like Burton's tar that has not been distilled?

A. A part of his has not been distilled.

Q. Exactly. Now, what does he do with that, the part that is not distilled in the first step?

A. He says that it can be recracked.

Q. But he does not redistill before recracking?

[fol. 673] A. Not in that case.

Q. Exactly.

A. (continued). He does in the other.

Q. Yes.

Mr. Rauber: You mean he does not say he does?

The Witness: He does not say he does.

Mr. Wiles: Some of our own patents do that. That is, we distinguish between pressure tar distillate and pressure distillate bottoms.

Q. You know what I would mean by both of them?

A. I am not certain that I do.

Q. Well, now, by pressure tar distillate, I mean the distillate obtained from distilling pressure tar, which has been left in the cracking operation; you understand that?

A. I think so.

Q. Pressure distillate bottoms is the heavy ends of the pressure distillate which have gotten over through the runback and are too heavy to go into gosoline. You understand what that would mean?

A. I think so.

Q. (continued). If I used those terms shortly?

A. I think I would.

Q. Now, it appears from certain of the defendants' patents that both of those are sometimes put into charging stock, that is, both pressure tar distillate and pressure distillate bottoms. Now, distinguishing between the two, pressure tar distillate and pressure distillate bottoms, which of those two is it that you say Noad re-cracks or makes at all?

Mr. Rauber: As I understand it, they are both the same, chemically?

Mr. Wiles: No, we do not consider them so at all. We consider them as different products, we handle them different.

A. I would say that he is making and using considerably more of the pressure distillate bottom—no, of the bottom whichever one——

Q. Pressure distillate bottoms is right.

A. (continued). Pressure distillate bottoms than Burton would be using, because his material is one which takes off all of the vapors instead of using a run-back.

Q. You do not find any pressure tar distillate in the Noad patent at all, or anything approaching it?

A. It is the same material, but in the case of Burton, it has been returned by the run-back, and then is distilled [fol. 674] over separately in a separate process; whereas, in Noad, he combines the two and gets that distillate at the same time he is getting his first cracking.

Q. How can you tell that it is the same product?

A. If you had here a product which would be—there is no particular reason, for example, to think that the atmospheric distillation changes the quality of the product to any great degree after it has been through the cracking process.

Q. Now, do you believe that the claims of Noad as to his results, are correct; in fact, his process will accomplish what he says it will do?

A. He is concerned considerably with the question of his iron filling, which acts as a catalytic agent. I am not at all certain that it does act, to any great extent, as a catalytic agent. The other parts there, I think, are correctly stated.

The one portion about which I have the greatest doubt in my mind is regarding how much of a catalytic agent "the portion which is packed with scrap iron, or otherwise adapted to act in a considerable area of iron in interstitial passages—"

Q. Do you think he would get the same result if he did not pack with iron?

A. No, I do not know as I would say that. I say I am not at all certain as to what degree that would affect the material. I would not expect it to affect it a great deal.

Q. Now, he also says, "the process is of advantage in relation to known processes, in that it enables a much higher percentage and practically the whole of the oil to be converted into volatile spirit and avoids the production of more than a very low percentage of permanent gas and residual carbon"; is that statement true?

A. It sounds like the customary statements of value of a process which are made in patents. I presume he refers there in this question of converting the whole of the oil into volatile spirit to the fact that he proposes to run his residue and bottoms.

Q. Well, now, as a man skilled in the art, do you think that is true in point of fact; that by practicing this Noad process you can take North American Solar oil of .863 specific gravity, which would be approximately 32.5 Baumé, and convert practically the whole of it into a volatile spirit suitable for use in internal combustion engines, and avoid the production of more than a very low percentage of permanent gas and residual carbon?

A. It may be. I have not tried out the process.

[fol. 675] Q. What is your opinion of it, Doctor?

Mr. Rauber: Did you ask him whether it was true or not? It seems to me your question is——.

Mr. Wiles: I asked him whether it is true, because one of two things must be true; if it is true there is a process for the art to use, obviously, and there is no reason for anybody taking any of our licenses, if that statement is true. If it is not true I want to know it, because it leaves the whole business in a realm of speculation.

The Master: He says he has not tried it out and he does not know whether it is true or not.

Mr. Wiles: What?

The Master: He said he had not tried it out and he does not know whether it is true or not, or to what extent it was approximately true.

Q. Can you tell, Doctor, as one skilled in the art, whether that is a true statement and can safely be relied upon without experiment or the necess-ty for experiment by those skilled in the art?

A. It is impossible for me to tell whether this is a true statement or not. I would expect that it would be subject to the normal limitations of patent disclosures and that it would have the same troubles that a patent is likely to have in the way of a man taking one of them and starting in to operate.

Q. No, as one skilled in the art——

The Master: It would require the same kind of experimentation?

The Witness: Yes.

Q. As one skilled in this art, do you believe that the Noad process is a practical, commercial, feasible process for the manufacture of motor spirit, in large scale, successful operations, I mean?

A. You mean profit paying necessarily?

The Master: Oh, no.

Mr. Rauber: Just whether it is operative or not.

A. (Continued). I have no question in my mind but what, using the Noad process, you can make gasoline and make it in quantity; whether it could be made profitably or not, I do not know.

Q. How much would the losses be in gas and carbon?

A. As I say, I have not operated Noad's process and I cannot——

[fol. 676] Q. Have you operated any processes of a generally similar character?

A. No, not closely similar to this.

Q. Now, Dr. McKee, will you continue your answer as to which of these references have the steps, separately, as set forth in this Burton process? I want single references that have those three distinct steps.

The Master: You not only want to have them for the three distinct steps, but you want them in the order described in the Burton patent?

Mr. Wiles: Yes, your Honor. I don't see how they could be otherwise, but maybe they could. I hadn't thought of that as a possibility.

A. I do not find any described in the words that you have given there, in Noad, British patent, of 1908, but I do find described in what a chemist would consider as being essentially the same, a process of this character. He does not describe it in the three distinct steps that you mentioned, but he does describe it as three steps.

Q. Which three, please?

A. The cracking process, the heavy products that have been distilled at atmospheric pressure, and condensed, and then the cra-king process—the cracking process being without pressure .

Q. That is only two steps, isn't it?

A. No, it is three—it is the cracking process and condensation, and a cracking process.

[fol. 677] Q. Burton, by that same token, has six—that is, Burton distills and condenses under pressure, and then distills and condenses without pressure and then distills and condenses under pressure again, doesn't he?

A. Yes, and if you translate it that way this man would have five.

Q. This man only has distillation and condensation the first time, and distillation and condensation the second time?

A. No—distillation and condensation, rectification, distillation and condensation,—five—meaning, by distillation, cracking in the first and fourth of Noad.

Q. Now, are there some other references that you say have these same steps?

A. Barber cracks under pressure—I read from patent No. 322,265—he cracks under pressure and distills off from his oil at near atmospheric pressure—or practically atmospheric pressure—perhaps three pounds pressure—in the column Q, and runs that product, which includes his gas oil portion, over to a second column, where the gasoline and kerosene are removed from it, and the gas oil returning

through the still end, and finally, again, into the cracking retort, P. He uses the same retort for both cracking processes. Whereas, Burton, may or may not have used the same retort for the cracking processes.

It is practically identical with Burton, because you have cracking under pressure, you have distilling at atmospheric pressure, and you have brought it around and cracked it again under pressure. Your product, that you have distilled, is not all of it as it is in Burton's—that is, only a portion is distilled. That is, Barbet condenses out and saves his paraffin and lubricating oils.

In Testellin and Renard, patent, No. 16881, of 1908, we have a cracking under pressure, a collection of heavy products in a vessel, A, and a re-working of this by cracking again under pressure.

Q. Now, has that got a tar or residuum, which is later distilled, and the distillate from that, re-run?

A. He is speaking of cracking illuminating petroleum to get lighter products, and, apparently, this kerosene is broken into two products—one a light product, which corresponds to gasoline, and, simultaneously, he gets some heavier products, but from the temperature used—450 degrees Centigrade—we would expect the kerosene to go over.

[fol. 678] In other words, it distills and is condensed at atmospheric pressure.

I read from line 9:

"It has been ascertained that the liquid, after being subjected to distillation, and fractioned, has changed in density. Light hydrocarbons have been formed contemporaneously with heavier congeners, by passing the heavy products through the apparatus again, they are finally transformed into light spirit."

This would be a process in which first we are cracking under pressure, we are not using a run-back, and consequently we are distilling off the product, which, in this case, is light enough to distill, and is caught in the container, A, and then that product is re-cracked. It carries it out strictly as a two-operations process, instead of a three-step process, but it is, to all intents and purposes, the same.

Q. Does he, in that, re-run any tar that was left in the still in the first operation? I mean, re-run it at atmospheric pressure and get distillate off of it?

A. He is using, as his starting material, kerosene, and, apparently, he does not accumulate an appreciable amount of tar in his first still.

Q. Then, he hasn't any tar in the first place?

A. It is tar in the sense of being a residue—it is a heavy product.

Q. But there is no tar left in the still, as Burton has?

A. He is using a raw material at a high enough temperature, so that he distills out his tar simultaneously. He probably has some coke.

Q. That is quite different from Burton, isn't it, because Burton leaves behind a tar in the still, doesn't he?

A. This man combines Burton's two steps into one—yes; it is different to that extent.

Q. He does not start with the same stock, at all, does he?

A. What stock does Burton start with?

Q. Well, what does he start with?

A. This may says that he can start with—he particularly describes this portion when he is starting with kerosene.

Q. Burton starts with a stock that does leave a residuum in the still, doesn't he?

A. I think that Burton and this man both state that they can use a heavier petroleum than kerosene.

The Master: Wouldn't this suggest something in relation to the kind of stock that Burton starts with: "This [fol. 679] method of distillation is continued until the residue in the still is reduced to a thin syrup-like consistency, and the original charge has been reduced from thirty to forty per cent of its bulk." If he was using crude, or anything approaching crued, would his residue be thin and syrup-like?

The Witness: Yes, as you crack your residue becomes more syrup-like—more thin, and more like the twine oil of his later patent—no matter what scheme of cracking you use—provided it is a liquid phase cracking.

Q. Do Testellin and Renard have any such substance as that in their still, at all, when they finish?

A. No, they combine Burton's first and second processes.

Q. You say they combine—they get overhead, then, what Burton leaves behind—is that your understanding and your position?

A. What does overhead mean?

Q. They get over as a distillate—overhead, of course.

The Master: They continue their first distillation until they get the products which Burton leaves behind—is that what you mean?

Mr. Wiles: Yes, your Honor.

A. Yes, and that distillate includes the product which Burton gets on his second distillation.

Q. And the first, too?

A. And the first, and then they separate the two by partial condensations and re-run the heavy product—the residue products.

Q. Well, if that is your position, let us have your next reference.

The Master: That is one of the difficulties, it seems to me, in comparing a part of these patents—that is, anyone not skilled in the art would suppose that everyone started with the crude—which is not so, at all. Many of these processes apparently deal with certain parts of the crude, which had been processed off of the crude before the operation starts.

The Witness: You can crack almost any oil, in your smaller, or later products, if you care to.

The Master: For instance, like a corn mill—you, naturally, would suppose that you would start with ear corn and shell it off of the cob, and then cream it, and then send it to a reduction mill, and germinate it, and reduce it to the size of grits, and then, another man starts off with grits.

Mr. Wiles: That is exactly parallel, your Honor.

A. Oglobin Zeitschift fur Angewandte Chemie, 1905, page [fol. 680] 526, does not use pressure,—other than that he cracks first at ordinary pressure and then he takes distillate and fractions it, and takes one portion of it and again re-cracks it. This is not a re-cracking of the residue, but a re-cracking of a portion of the product of his first distillation.

Q. Re-cracking part of the distillate, rather than part of the bottom?

A. Yes, in this particular case.

Q. And that is true of all these references that you have stated so far—they have re-cracked part of the distillate, instead of part of the bottom?

A. That is simply a question of terms, because any bottom that is distilled, as Burton does, in his second step, becomes a distillate, and they have done the same thing.

Q. Does that exhaust your references?

A. That is all I have here on this memorandum.

Q. Now, in the course of your testimony you referred to certain devices that had rotary scrapers in them. Those are designed to be continuous coking devices, aren't they, for the continuous coking of heavy tars or residues, at atmospheric pressure?

The Master: That first came in the Adams patent, didn't it—some Adams patent?

Mr. Wiles: I think so, your Honor. His references on that line were Gaster and Lennard.

A. I am not certain whether Inschick's wire discs, in the form of brushes, placed in his retort, rotated, or were raised up and down. In that particular case it is not a coking process.

Q. The other two, Gaster and Lennard, are, are they not?

Mr. Rauber: Wait a moment. I don't think he understands you. What do you mean by coking—a dry coke?

Mr. Wiles: Yes.

Mr. Rauber: Do you understand it that way?

The Witness: I did not, quite, but I catch it a little different, now.

The Master: Those references were directed to Adams, No. 1,327,263—the spiral rotary scraper.

A. In the case of Gaster we have the use of his scheme on a tar still of complex type. But is the case of Lennard we do not have the running of the still as a coking process, as is done in the case of petroleum coking.

We have, in this case, a still where the tar is diluted with naphtha, and it distills over, and there is a simultaneous, [fol. 681] running in of stream, and a separation out of carbon, which has been held suspended by the tar.

There is, apparently, no large new production of carbon in the process.

Q. In both of them do you get other residues—you distill off everything distillable, don't you?

A. You do not get a dry residue from Lennard. The product drops down below, as he speaks of it there, and it is a wet product.

Q. Now, do you consider this Gaster continuous coking device as a practical and a commercially usable device for its own intended purpose?

A. I think so. Of course, in this case, as with any other citation, a man could probably operate it so that it would not work, but it would probably work, and work fairly well.

Q. What do you know about the effort, if any, of the oil industry, to discover a feasible continuous coking process?

A. I know that there has been conversation along that line, and some work, but how much I——

Q. Don't you know, as a matter of fact, that the thing most searched for in the whole oil industry, for many years, has been a continuous coking process, and that they have never gotten one that will work commercially?

A. I know that there have been conversations and articles written on that process, but I don't understand that this was cited as a process for a continuous coking operation.

Q. Well, that is what it is, isn't it?

A. Not primarily.

Q. Gaster?

A. No, it is not.

Q. Doesn't it takes its oil—whatever it is, crude or anything else—and give you a hundred per cent distillate, with the exception of dry coke that is left in the apparatus, which is scraped out and delivered out at the other end?

A. It gives you, in part, that, but only in part.

Q. What else does it give?

A. For example, this is primarily for products which carry free carbon in suspension, and in your coking process that you refer to, there is carbon produced in considerable amounts, during the coking process.

Q. If you used crude or tar, or any of the things—or most [fol. 682] of the things that Gaster states—it is a continuous coking process, isn't it?

A. Yes, in one sense, and no, in another.

Yes, in the sense on the question of the removal of suspended carbon.

No, in the sense of being primarily for the removal of carbon, when carbon is produced in the process, to any great extent.

Q. Now, if you take crude oil, do you mean to say that you can put it through that Gaster device and not form carbon in large amounts?

A. He doesn't run that clear to dryness, as is indicated on page 8:

"The carbon scraped off of the walls of the retort, 1, drops into the intermediate chamber, 23, provided with a sealing door at the top, and one at the bottom—not shown—which doors may be mechanically connected in such a way that one of them must be closed before the other can be opened, so as to prevent the access of air to the retort."

Then he gives an alternative method. You would have, here, your carbon worked out with heavy products, which would be quite unlike a coking still of the petroleum industry.

Q. What is there in that that indicates that the tar is not dry?

A. Because it has been in contact with the retort and drops down there at the time that the product is being distilled, and, as such, it is wet with oil, and that lower portion would be full of oil, of course.

Q. It is wet just the same as you said Lennard was, then?

A. It doesn't happen to be just the same, but it is also wet.

Q. You will notice that, on the contrary, Lennard says that his comes out as a rough powder—do you mean that that is dry, or wet?

A. And he says, further, that it——

Q. Well, can you answer that? Does Lennard's come out dry, or wet, when he says it comes out as a powder?

A. Line 77 of Lennard:

"C is a pipe, and 1, by which steam is admitted and projected upward from the lower part of the chamber, or retort, B, the bottom of which is provided, there, with a water well, B-3, constituting a seal, into which the carbon [fol. 683] falls, and from which it can be removed as required, through the outlet C."

It would hardly be a dry powder, falling into a water well.

Q. Well, he has the alternative form, there. Do you mean that that carbon comes out free from oil, or not? That is, is it dry before it goes into the water?

The Master: Wouldn't that depend on the extent to which he carries his process?

Mr. Wiles: Well, it is a process, and I want to know what the process is. If he gets it out of the dry carbon, I want to know it—if that is his position—free from oil.

The Master: What I meant to say, was, whether it comes out dry, or not, would be dependent upon the extent to which you continued to distill your residue, until you have practically reduced it to a dry coke—or a relatively dry coke.

Mr. Wiles: But this is not a batch process. It is a continuous process, in which the stuff is sprayed on the wall, and scraped off. It is a description of the process, and I want to know what happens in the process. Does it drop down as dry powder, or doesn't it? What does it do? It would be entirely two different processes, as a matter of fact—a coking process that really cokes, is one thing, and a process that runs down to a heavy tar bottom, is another process entirely, and I want to know which this is. I am asking the witness which it is. Does Gaster give dry carbon, free from oil, or what does he give?

The Witness: Do you want Gaster, or Lennard? You asked me about Gaster.

Mr. Wiles: Lennard is what I meant.

The Master: What is the lowest side of his residue, is what he wants to know.

The Witness: The lowest side of his residue would be wet with water—for Lennard.

Q. In one form, and in the other in what?

A. In the other one, as he heats only to 500 degrees Centigrade, it is probable that he would not volatilize all his heavy oils of his tar, and in that case it would be wet with oil; for tar has been distilled up at a much higher temperature, and carries some of the products which, probably, would not distill at that temperature.

Q. And that is in spite of the fact that he says it is a powder?

[fol. 684] A. I have looked at this patent, since you spoke, and I fail to find the word "powder" as you spoke of it.

Q. I find it on line 38, page 1.

A. "The free carbon being deposited in the form of a rough powder."

That is, it would be deposited on the sides of the retort, but probably wet with the oil which was being sprayed into the retort, but that doesn't mean that it was a dry powder, necessarily, at the end. Though it might be, its being sprayed with oil, and its being heated only to a temperature at which we would expect only a portion of it would be volatilized—that is, 500 degrees Centigrade.

Q. And your position is that with 500 degrees Centigrade, which is 932 Fahrenheit, you would have liquid oil left, if it was sprayed on that surface and trickled down the walls?

A. It is my position that if this was falling down, with it would go vapors, which would condense on it, and we would have it damp. But his regular process is where he uses that with a water well.

Q. With a water well is the carbon dry before it reaches the water well?

A. I don't think so.

Q. You think it is wet with oil?

A. Somewhat.

Q. With this oil that condenses on it, at 932——

A. But you would have, as that was dropping down in the cooler zone, condensation of vapors with it.

Q. You have got steam going up through this, all the time, haven't you?

A. Steam, yes, some—at least in one alternative there is steam.

Q. Now, in your judgment, would it be possible to have any oil condensed in an inverted conical receptacle, with a temperature of 932, and a current of steam going up through it?

A. Do you refer to the chamber, B, of the patent?

Q. Yes, sir.

A. The patent, on page 2, line 6, says:

"This mixture, after being slightly warmed by the steam coil, is then fed, in a small stream, into the retort, hereinbefore described, which has been heated to a temperature of 500 degrees Centigrade."

Now, if it was being fed in as a stream, there would un- [fol. 685] doubtedly be oil striking the sides, there, and being scraped off with the carbon.

Q. At what point?

A. Where the oil struck the sides and the carbon was scraped off, shortly thereafter.

Q. Well, the oil strikes the sides at the top, only, doesn't it—that is the layout, isn't it?

A. Entering by the pipe, A, it would pass down over the spread, B 2, and so on to the retort, perhaps half way down.

Q. Isn't that rotated, and intended and designed to throw the oil onto the upper part of the retort?

A. I wouldn't say, from that kind of a gear, that it was expected to rotate, at all, rapidly—it is not a high speed gear.

Q. Isn't it the intention of this device to have the oil spread over the upper part of the cone of that retort, so that it will work down in a thin film, over the whole surface?

A. To a considerable extent, but not exclusively.

Q. Well, what kind of oil is there, that you know of, that can remain liquid in a current of steam, at 932, if on an iron surface of that kind?

A. For how long?

Q. For any length of time, within reason?

A. A small steam of oil coming in here and falling to the surface, would, for a short time, remain liquid—that is, there would be evaporation off, and there would be local cooling, due to the evaporation of your naphtha, so that you would have, for a little while, a considerable portion of it, as oil. If you would stop your feed it would soon dry up, but continuing your feed continuously, you must have liquid oil on your surface of your carbon at the time when the scraper is striking it, and consequently it would carry it down.

Q. And whatever liquid is there would simply slide down along the surface, until it is dry, won't it?

A. If it was scraped off, it would probably fall rapidly, as the solid would fall rapidly in the atmosphere of vapors and steam. There is nothing, particularly, to hold it up.

Q. Now, you think, then, that this kind of a process, operating as described, can be commercially operated, or

can be operated a reasonable length of time, and keep the sides clear, do you?

A. I would say that it can be.

Q. How long do you say that this Lennard process can be [fol. 686] operated with the retort held at 932 and keep the sides clear, by means of this scraper?

A. It is impossible to say. One operator can operate it longer than another, and it would depend, a good deal, on the skill of the operator.

Q. Now, what would you do if you were to run it? I want to know just the regime that you think would give us the longest operation?

A. What kind of a feed do you give it?

Q. Any kind you choose, within the Lennard disclosure. How could you run that with a retort of 932, and keep that clear, for any decent length of time?

A. It is to be noted that this is a scheme which works at small capacity—that is, a small stream is fed in—and we would have, there, the working of it, perhaps, without difficulty, for a considerable time. After awhile I would expect carbon to be formed on the original shaft, and on the other portions, there. At that temperature, radiated heat would come in to affect it, to some extent. But it is probable that such a process could operate, without particular difficulty, for probably a week, if given a good operator continuously, and provided it was not over-fed.

Q. Just what would the operator do to get a week's run out of it?

A. It would depend a good deal on the character of the operator. We must presume on that—that we have a man who was trying to operate it, and not one who is trying to not make it run.

Q. If you were trying to run it and make it run, just what would you do to get a week's run out of it? Give me your running orders, that you would give to the most skillful and conscientious operator that you could get—how big a device would it be that you would tell him to use?

A. In the first place, there would be some experimental work that would probably have to be carried out. In the first place, we would heat it up and test it to see if there were any leaks.

Q. How big would you make it, to start with?

A. Let me get through with this, and then I will come to that.

Q. Well, that was in my question.

The Master: He says he cannot answer it all at once, and he will answer it when he comes to it.

A. We would then start up, after we found that it was [fol. 687] all right, mechanically, and right from the question of leakages, and essentially free from leakages—we would then start up, using a small feed, and a somewhat lower temperature than we expected it to operate under. In other words, we would try to reach a stage where the carbon would not be adherent to the maximum extent.

The next step would be, that we would, perhaps, raise the temperature, continuing to raise the temperature until we began to have trouble about carbon adhering. Then we would come back to a somewhat lower temperature, so that we would get a fairly ready removal.

All of this would be somewhere in the range of approximately 500 degrees. Of course, we wouldn't have, at any time, all of the parts at the same temperature, and the temperature of 500 degres, which the patentee mentions, is, of course, only an approximate temperature.

When this temperature had been reached, and by the insertion of a pyrometer, at the particular point, and a record made of it, we would then run it at that speed, or temperature, or, perhaps increase the speed somewhat, to see if we could give it larger capacity—though so doing would mean more oil going out to the carbon.

When that was done we would continue the operation for a time—for as long as we wanted to, provided we didn't have trouble with carbon forming on the shaft, and elsewhere.

As to the size of this, it is probable that a retort of this kind, with a scraper there, should not be made of too large a diameter—for two or three reasons.

In the first place, it is difficult to heat a large vessel without it warping, and, as such, giving trouble, from the question of the scraper running on the surface.

And I would suggest what I think I would be inclined to start with, which would be, perhaps, a vessel say five or six feet in height and two or two and a half feet in diameter.

This, of course, is an attempt on my part to operate a process disclosed in this patent. It is subject, and probably would be subject, to a good deal of trouble at the start, because, always, in starting up a new process, we have troubles which we have not foreseen—perhaps troubles in the fire box, perhaps troubles in the blades, or in other phases.

[fol. 688] Q. A fluid, at the bottom.

A. I would run it with the expectation of getting a rough powder, but still damp with oil.

Q. And you think a process that gives you essentially a rough powder, dampened with oil, could be run as you have described, for a week?

A. I would expect so. I don't mean to say that such a process would, perhaps, pay commercially in a single unit.

Q. Now, how much feed would you be able to give this 5 by 2½ foot device?

A. I told you that I didn't know, and that I would have to start up and try, to see what the capacity of it was. I said that in my suggestions for operations.

Q. Now, in the course of your direct examination you were asked by the Master, somewhere, whether this Lennard device was suitable for use with pressure, and you said it was.

Did you mean that the apparatus, just as it is described here, was suitable for use, without any changes?

A. With only the changes that any chemical man would make on it.

Q. The device, as it is illustrated and described, is what I would like to know about, and I suppose the Master does.

A. I wouldn't think that this would be—if I were operating it, as I said in the beginning, I would put in a pyrometer and take the temperature, and I would give it a good foundation, and I would do half a dozen other things—just the things that anybody operating such a plant, or apparatus as this, ought to do.

Q. Taking it just as it is, and taking Lennard's process, laid out just the way he wanted it, could you put pressure on that device by just closing down the outlet, or would you have to rebuild and redesign it?

A. The retort, itself, would probably stand a reasonable pressure right to start with. It is, apparently, a cast iron—or maybe a cast iron retort. But, nevertheless, with

the size that it is, that I have indicated, and with its general shape, there, it would stand some pressure. It would not stand high pressure. If it was wanted for higher pressure we would make it more curved at the top, instead of having so flat a top as this is. Similarly, we would put in a better gland at the top of the apparatus, and at the bottom. His scheme of removal of the product is not indicated, definitely, except in the case of the use of the water well, but it would probably have to be changed. For ex- [fol. 689] ample, the box, B-4, would be made a different shape, because here it is flat on the side, and flat sides do not stand pressure near as well as a cylindrical vessel. There are a number of things of that kind, that would be changed, if we had any considerable pressure. We can have it under small pressure without making any change, or essential change, but so far as the point which Lennard is describing and disclosing, I should say it was clear enough as to how it can be changed in order to use the pressure.

Q. Any substantial pressure at all, would it not?

A. As I recall, the Redisch plant uses cast iron vessels of as large, or larger, capacity than those under pressures of four atmospheres successfully, and this could be used, though as I say, I would prefer to use one with a heavier and more curved top.

Q. This carbon removal scheme could not — performed under any such pressure as that, could it?

A. Yes, it would be perfectly possible. We do have conveyors working under such pressures as that.

Q. Now, what he says is that he has some sort of a——

A. He has a conveyor working at *b*3; that is probably a conveyor.

Q. What?

A. I say, at *b*3 he has his water seal.

Q. He says he has an air-tight receptacle, *b*4 as shown in the dotted line in that figure?

A. That is his alternative; the other case is where you have got a water well.

Q. A water well is just a water seal, is it not?

A. Apparently it is a little more than that, because he speaks of its removal through the outlet, *b*3.

Q. Yes. In other words, it is just down there in a tub of water, is it not, and he can rake it out under the water?

A. He says quite differently.

Q. How do you understand that to work?

A. He says B3 is a water well. Now, that is not anything like a tub of water; it is a column.

Q. What do you call B4?

A. (continued). And that *b3* is an outlet for it, so this is the alternate method with an air-tight receptacle there, B4, box; that is, you have got two schemes which can be used, but if you are going to use, as I said a moment ago, an air-tight box, B4, and you wanted to make it under pressure, you would not make it with flat sides, as it is here.

[fol. 690] Q. If you wanted to get carbon out of that thing under pressure, how are you going to do it with a water well; let us take that first.

A. If we would put, for example, a barrel valve where *b3* is, and rotate it slowly, it would take out its load of carbon, quite wet with water each time, and without changing the pressure. It would take out some water, so that there would be a supply of water put into the water well, B3, from a pipe.

Q. You do not see anything like that in this patent, do you?

A. You are asking me how I would do it, and I am telling you.

Q. How would you do it with the apparatus that is suggested here in the patent?

A. I have told you that I do not understand that this man attempts to tell everything that is here. He does not, for example, make complete foundations for his apparatus; he does not put in thermometers; he does not put in level gauges, and all of those things; but to put it under pressure, you could. You asked me how I could do it, and I am telling you.

Q. In the first place, the Master asked you whether this could be used under pressure, and I supposed he meant as shown in the patent drawings, and disclosed; certainly, that is what I am curious about. Now, I have tried repeatedly to ask you whether this could be used under pressure without change from what is here shown and described, and if you will just answer that question, why, we will get along. Each time you come back and add something to it. Now, can the thing here shown be used under pressure, a substantial pressure?

A. Yes, if we would give it the accessories which anyone skilled in the art would do, normally, when wanting pressure; that would be a pressure gauge, and they would do other things with it.

Q. Now, suppose you take this air-tight box and this top that he shows here, how are you going to get your coke out of that air-tight box without losing your pressure every time you take it out?

A. As I said a moment ago, I would prefer the water well scheme, the alternative scheme, if we were using it under pressure.

Q. How would you get it out of the box, B4, if you had to do it without losing your pressure every time?

[fol. 691] A. You could do as I suggested for the water well, put a barrel valve on it at the bottom, and that valve would take out—it would fill when the mouth was open upwards with the carbon, and then, on turning it over, the pressure would not be changed. There would essentially be no leakage and you would have an emptying out of the carbon from it.

Q. Do you think you could make a valve of that kind run with coke in the form of powder and stand up?

A. For a time.

Q. A week? Stand up through one run of this week that you have been talking about, with coke, the whole pressure?

A. I think we have barrel valves that do do that on very similar materials.

Q. Where are they used and what kind are they?

A. You have them described in works dealing with mechanical appliances, in the mechanical engineering field. I think you will find them in Sweet's catalogue.

Q. What industries employ such valves and what pressures do they work under? employ such valves on granular or rough material?

A. I think you will find also a list of industries or cases where they are used on rough material.

Q. Under pressure?

A. It is a scheme which has little use excepting under pressure. The pressures may not be 300 pounds but the pressure- are distinct pressures.

Q. Well, more than a half a pound?

A. Oh, yes.

Q. Will you please just tell us one industry where that is done in commercial operation?

Mr. Rauber: Your Honor, I want to object to the question. I think it is entirely irrelevant and immaterial, way outside of the issues. What industries these valves may be used in has nothing to do with this case, as far as I can see.

The Master: Counsel asked the witness if he could suggest such valves and he said he thought he could. I do not suppose we can go much further than that. He is not supposed to know; he is not engaged in the crane valve industry in making valves and selling them.

Mr. Wiles: No, I am asking him the place where such a valve is actually used.

The Master: He will probably say to you go and get a catalogue.

A. One place I think I can give where it is used, but per- [fol. 692] haps not as high pressure as you want there, and that is on the feeding of gas producers.

Q. Feeding what?

A. Gas producers, where gas producers are fed, coke, or where they are fed anthracite coal.

Q. They are run under a vacuum, are they not?

A. Not always.

Q. Usually?

A. Well, sometimes they are, sometimes they are not. Sometimes they are under distinct pressures.

Q. Never more than a half a pound?

A. That was the pressure I believe that you mentioned, a half a pound.

Q. Yes.

A. Sometimes that much, sometimes I think more than that. Sometimes the seal on them is perhaps about a half a pound.

[fol. 693] Q. Now, you stated a little bit ago, in talking of the Gaster patent, that it did not deliver a dry material. By that you mean that the material is damp as it comes out, or that it comes out as a liquid? What I am getting at: Is it a damp powder or a real powder that is supposed to come out of there?

A. Apparently, it is mixed with some of the volatile materials. For example, page 12:

"The carbonaceous product obtained by my process has the shape of an exceedingly fine powder consisting of car-

bon mixed with a small quality of volatile matter. Owing to its great purity and fine structure, the said powder may be used directly, that is to say, without previous calculation, for the manufacture of arc lamp used in electrodes, but in the manufacture of electrodes of great diameter it may occur that the calcination of finished or moulded electrode would cause considerable contraction, in which case it may be neces- to calcine the fine carbon powder, so as to drive out most of the volatile matter, before the material is formed into electrodes."

The fact that it speaks here of volatile matter left in it, which is immediately mixed with carbon to dry, in order to get it into a viscous mass that can be moulded, would imply that this was wet—that the fine carbon particles were wet with a small proportion of organic oils.

Q. Do you understand that would be dry in appearance at ordinary atmospheric temperature, or essentially damp as it was delivered from the apparatus?

A. I would say that it was probably, as first obtained, damp from the steam which he suggests here, to some extent; but that on drying you would have a material which would lump up if you squeezed it, but it would be capable of being shoveled and would not adhere particularly to the shovel. In other words, somewhat damp with the oil.

The Master: When you speak of dry coke in this industry, you do not mean dry as comparable to the dryness of gas coke, do you?

The Witness: Yes, I think so, in general. That is, in the petroleum industry there is coke that is dry, there is also coke that is not. I mean, it would depend upon how you would operate your dry distillation. In this particular process you have here a somewhat damp coke, that is, dampened with the heavy oils.

Q. You have another case, have you not, also, and that is that the coke carbon has mixed with it a very small percentage of pitch, which is so high melt that at atmospheric temperature the whole thing is essentially dry, but that when heated up a little will volatilize?

A. That is also possible.

Q. And this might be just the case with coke that is essentially dry to the touch at ordinary temperatures, but has in it a very little high melt pitch?

A. In reading this as a whole, I would not think so, though that is a possibility.

Q. Well, the thing that would naturally be left unvola- [fol. 694] tilized in any of these processes would be the extremely high melt material, would it not?

A. If he was distilling a tar, a product which had already distilled—I mean from coal, he would perhaps get such material; if he was distilling some other of these hydrocarbon mixtures that he mentions we might not. It would depend a good deal on the raw material you start with.

Q. Well, if he started with anything like crude oil the only thing that can be left, unless you have a perfectly dry coke, are these very high melt pitches, are they not?

A. I do not so consider it, because here, as it drops out from the heated zone, in here (indicating), we have it best shown in the figure, the products drop down in the vessels 23 and 24, where undoubtedly you have vapors but vapors that are cooled, and as such, they would condense on the particles and on the walls and you would have condensations of the same vapors that are being distilled, or in other words, light boiling hydrocarbon oils. When I say "light boiling hydrocarbon oils", I mean such for example as kerosene or lubricating oils.

Q. You do not think that would be warm enough down there to prevent condensation?

A. Not such a product as lubricating oil, for example.

Q. Even with steam passing through?

A. Let me finish here.

He speaks of running a truck, placed directly underneath it, page 8, line 22:

"Instead of two sliding doors, other suitable devices may be adopted for excluding air from the intermediate chamber. Carbon is allowed to drop into a truck, 24, placed underneath the same."

That is, with such an apparatus as that, you would, of course, not have it very hot. Men could not work around it with a truck if it was, and the truck itself might catch fire and burn. This was a wooden truck.

Q. Dr. McKee, I show you an article in the Chemical Age of January, 1922, entitled "Gasoline from Shale Oil", purporting to have been written by one Ralph H. McKee of Columbia; is that your article?

A. I think so.

[fol. 695] Q. In this article is this statement:

"In New York it is not uncommon to have put in one's car a gasoline carrying twenty per cent of unsaturated constituents, which means that such gasoline on standing and in contact with air discolors, and separates, after a time, a small amount of brown tar-like deposit."

Is that statement correct now?

A. I do not think it is so true as it was in 1922, though at the present day we often do get, or can get, in the State of New York, gasolines carrying twenty per cent of unsaturated constituents and which become, on standing, somewhat discolored. I do not know that they separate brown products, but they do (some of them) separate yellowish products and give yellowish color.

Q. Was that statement generally true to the best of your knowledge and belief in 1922?

A. I do not believe there I say it was generally true; I said it did happen.

Q. And it was true when you made the statement, according to the best of your knowledge, information and belief at that time?

A. Yes.

Mr. Rauber: What is true? You mean some of it might contain brown tar-like deposit?

Mr. Wiles: He says "it is not uncommon." The statement is, "It is not uncommon."

Q. Now, I call your attention to the following paragraph on page 2, under the heading, "Dr. William M. Burton":

"The cracking process most largely used is that devised by Dr. William M. Burton, a chemist now president of the Standard Oil Co. of Indiana and the recipient, January 13, of the Perkin Medal, awarded by the American Section of the Society of Chemical Industry for notable attainments [fol. 696] in the industrial applications of chemistry. The Burton process is used by Standard Oil Companies and licensees with an approximate daily yield of two million gallons of gasoline.

Burton starts, as do all cracking processes with a cheap petroleum residuum, preferably of the Pennsylvania or

mid-continent types. The distinctive feature of his process is that he heats under pressure of 75 pounds and does not relieve this pressure until after the vapors have passed through the condenser. The oil is heated by direct fire in a steel still tank at a temperature varying during the process but which averages about 700° F. Any cracking process is handicapped by the deposition of coke on the bottom and sides of the still, the Burton process suffering severely in this respect. The original patent claims that the product is free from unsaturated compounds. However, the product as at present made does carry a considerable percentage of unsaturated constituents. The process is fairly cheap to operate but suffers as stated from heavy flinty carbon deposits, high deterioration of stills, danger to operators, and also because the products formed are too unsaturated to be of the highest grade."

In 1922 when this article was written, did you believe the contents of that paragraph to be substantially true, according to the best of your then knowledge, information and belief?

A. I then believed it to be true.

Q. And have you changed your views since that time; if so, state in what respect.

A. To some extent.

The Master: You are not required to go any further than the question.

Q. I also call your attention to this paragraph, under the heading, "Cracking Crude Shale Oil":

"If we are to have gasoline in quantity from oil shale we must make the crude shale oil and then crack it by some one of the oil-cracking processes. If the Burton process is used the gasoline so made will not be one of high quality. It will be strongly odored, will color on standing, and will be inclined to carbonize in the cylinder. However, its heat value per gallon will be greater and accordingly in a properly designed motor it should give a slightly higher mileage than old-fashioned gasoline."

Did you believe that statement to be true in 1922, at the time of this paper?

A. I discussed that with Mr. Neave. I believed it to be true, but I now know that shale oil gasoline can be and was

[fol. 697] made previous to that time, which was not strongly odored and highly colored, and that it can be made, is being made now.

Q. Is it not a fact that the crude gasoline produced in accordance with the process of the Burton patent, condensation being under still pressure, the process being otherwise conducted under that cracking process, contains a smaller proportion of objectionable unsaturated compounds than the product of a process similar in all respects except that condensation takes place at atmospheric pressure?

A. I have never heard of such being the fact.

Q. What is your opinion on it?

A. I would expect them to be essentially the same.

Q. In the respect inquired about in the question?

A. Yes, other things except condensation under pressure being—that being the only point of difference.

Q. When you say "essentially the same" you mean that the proportion of objectionable unsaturated bodies would be the same in the two products?

A. I said that I would expect them to be the same. In other words, if we took the condensation under pressure and varied only that at that point, at the beginning of a condenser, we ought to get over just the same materials because we are condensing the same things.

Q. Of course, the answer that you made to Mr. Neave, as to whether the cracking reactions were endothermic or exothermic—you stated that you would judge it was endothermic by reason of the difference in the heat of combustion of the products, that is, of gasoline as compared with the crude product.

A. I gave that as one reason.

Q. Yes. And the difference there is about 2,000 B.t.u.'s per pound, believe you said, or at least that is a fact, I do not know whether you said it or not.

A. I would like to have the text. I do not think I said that in that way.

Q. Well, you know what the fact is, approximately.

A. I considered it as an endothermic reaction, did I not?

Q. And endothermic to the amount of the difference in heat of combustion of gas oil and gasoline per pound?

A. No, I called attention to the fact that there were some carbon and some hydrogen given off at the same time.

Q. That is, a small per cent.

A. Well, it may not be. In some cracking processes it is [fol. 698] as much as 20 per cent for the amount of gases and carbon; in other cases it, many times, is as much as 5 to 10 per cent.

Q. Would you expect this endothermic heat of reaction to be in the neighborhood of say a thousand or two thousand B.t.u.'s per pound, if so, how much?

A. I have in mind the exact figures on it, but I know that the work indicates that it is an endothermic reaction of moderate character.

Q. Some reasonable per cent of the heat value of the products involved? The reason I asked 2,000 is that is about the difference between the heat value of gasoline and gas oil. Is it somewhere in that order?

A. As I said, I do not have the figures in mind. I would expect it to be really less than that, but I do not have the figures in mind.

Q. Of course, you can get the heat value of the products in your Kansas City Testing Laboratory book.

A. Possibly. If we knew just what process was being used, what raw material was being used and what product was being made; we -ould have to make a number of very decided suppositions.

Q. Now, will you please give me the very best estimate you can in the light of all present-day knowledge of the heat of reaction involved in the transformation of gas oil into gasoline? We will say per pound of gasoline produced.

A. I have told you four times within the last ten minutes that I do not have the figures in mind. The only way to give that would be to depend on memory of certain figures and calculate from them, and I haven't those figures in mind.

Q. How close can you come, Doctor, at this time?

[fol. 699] The Master: I think you have pressed it far enough. He has given you an answer.

Q. Could you calculate it from the Kansas City book?

A. It would take me probably an hour and a half to do so, possibly, for I do not know just what the Kansas City book has in it in that connection.

Q. Do you know of anything in the prior art, in the art prior we will say to 1913, '14, '15, somewhere that indicates whether this reaction is endothermic or exothermic?

A. Reactions in general which give gases, are en-

dothermic. That is, you have always entering in the latent heat of evaporation of your gas, and that, of itself, would make most reactions endothermic, even if it was a slightly exothermic at the time. That is a real item in such a reaction here, both the evaporation of the gas and the evaporation of the vapors.

Q. You do not know that——

A. (continued). Those are heat absorbing phases of the commercial reaction.

Q. Yes. I am trying to get at the heat, the chemical heat. Do you know anything in the prior art that tells you anythink about that at all?

A. I cannot offhand give you a citation, although I presume there is ample to be had if one looks for it.

Q. Now, I asked you some days ago for the fractionation of shale oil gasoline, a sample of which you brought in. Have you got that yet?

A. Oh, I had forgotten it.

Q. Now, you made, at Mr. Neave's request, certain iodine and bromine number determinations and in addition you made into a table you brought in some sulphuric acid determinations. What means did you use, if any, to keep your acid cool during these determinations?

A. By putting the bottle in suspension in ice and water, shaking it in that.

Q. How long did you shake it?

A. Fifteen minutes.

[fol. 700] Q. Now, you said on page 556, I believe, that De Clerq used a tube and tank process. The De Clerq process was at atmospheric pressure, was it not?

A. Approximately atmospheric pressure.

Q. That is not a cracking process, *it* it?

A. I would imply that it was. I would understand that it was, that is, that the coils were at a cracking temperature, as shown on page 3:

"Much time is saved as the great heat of the heating system rapidly expels the water from the top."

That is great heat.

"When the operator no longer hears the characteristic noise of steam in the receptacle, he knows that the tar has

given off all its water. He now runs the pump at full speed and the distillation proceeds. The furnace is in an insulated chamber so that in case a heating coil burns through, the tar from the coil flows over the fire in the pit."

If it was hot enough for a cracking coil to burn through, it would certainly be hot enough to be a cracking process, particularly as it is not under any particular pressure. So that would say those two will show that this patent, which is for a distilling apparatus, would show that the oil was to be operated upon at cracking temperatures.

Q. It runs on a water bearing oil, does it not?

A. At the start, until the water is removed, and then the product is re-circulated and re-heated higher.

Q. Do you think that that is an adequate disclosure of cracking?

A. He is disclosing an apparatus, and he is discussing primarily an apparatus, and he indicates its operation under cracking temperatures.

Q. Your view is then when a tube burns out, that is, the suggestion that a tube may burn out in that process is a sufficiently adequate disclosure of the fact it is cracking?

A. It is an indication that it is at high temperatures, high enough temperatures to do cracking.

Q. That is, that the whole oil body is at the cracking temperature or just the skin at the surface?

A. He says that the new process overcomes certain of the disadvantages of the old processes and one of them is these old processes have a disadvantage that the tar can only be heated to 400 degrees. As it is a German patent, consequently it is Centigrade. So that if this is a disadvantage, that the old processes can only be heated to 400 degrees, [fol. 701] then the new process is not subject to this disadvantage, having a higher temperature than 400 degrees Centigrade. If the tar is heated to above 400 degrees Centigrade it is certainly at a cracking temperature. He is overcoming this difficulty of the old processes in which the tar can only be heated to 400 degrees, but in some cases, they operate at less temperatures. He calls attention to it.

Q. Doctor, you, in your reported results on the 70 degree naphtha, showing 12.5 per cent absorption in sul-

phuric acid, with Hubl iodine of 3.4 and the Hanus iodine of 4.8; what is your opinion—your present opinion as to the reactions possible between the sulphuric acid and the paraffins?

In this connection I want to call your attention to your direct testimony at the bottom of page 348 of the printed record.

A. It is my impression that the probabilities are that this naphtha carried a certain amount of unsaturated, just as straight distillate naphtha, described in Allen's Commercial Organic Analysis in 1910 from American petroleum, which was supposed to be a straight distillation, [fol. 702] commercial product, had a bromine number of 18.3. This has a lower bromine number of 2.9. In other words, here we have a product which had probably a small amount of unsaturates there, so that I would say that this method (and as is commonly known, it is not an exact method) indicates here a certain amount of unsaturates, even though the shaking was done for but fifteen minutes at ice temperature.

Q. What I am getting at, Doctor, is this: The iodine numbers given for that particular sample show a very low percentage of unsaturates, do they not?

A. Yes. But I want to call your attention to something that, perhaps, in one of my papers, you may have missed.

I have shown in work published about 1911 that we can take, and with sulphuric acid not at ice temperature but at slightly above, at room temperature, or thereabouts, by very vigorous agitation and long agitation, and get our so-called saturated hydrocarbons to dissolve completely, or approximately completely. That requires simply extreme length of agitation.

On the other hand, as the test is normally carried out at ice water temperature, with only the excess given by the Bureau of Mines' method, we normally think of and it is so stated, that the paraffin hydrocarbons have, roughly, zero solubility.

Q. Your iodine and bromine numbers here would show only one or two per cent of unsaturates in that oil, would it not; a very small per cent?

A. It is quite a small percentage; but as I said here, we

shook vigorously for the fifteen minutes, and as such, we may have gotten results which were not exactly parallel.

I may add, that the two numbers, of which this is the average, were eleven and fourteen.

Q. That would indicate that instead of the paraffin hydrocarbons failing to react with sulphuric acid, in your test, you actually dissolved about ten per cent of the hydrocarbons in sulphuric acid, did you not?

A. Perhaps some.

Q. Well, it is in that order of ten per cent, in view of the iodine and bromine numbers is it not?

A. If the iodine and bromine numbers are to be given more value than the sulphuric acid, it would be somewhere in that neighborhood of ten per cent.

Q. In the course of your direct testimony when you were testifying about the Burton patent, the original, first Bur- [fol. 703] ton patent, you said there were some seventeen references of pressure distillation. You only cited a few of them, will you give us the list, please?

A. I find that my list has increased from 17 to 29 and they are as follows:

If I give you the number that is all you need, is it not?

Q. Yes.

A. Petitioner's Exhibit 98, Engler of 1897, six atmospheres.

Petitioner's Exhibit 99, Engler of 1888, four to ten atmospheres.

Petitioner's Exhibit 152, Engler & Hofer of 1909, 20 to 25 atmospheres.

Petitioner's exhibit 153, Engler, of 1893, 375 pounds.

Petitioner's Exhibit 151, Lobry DeBruyn, 1895, 15 atmospheres.

Petitioner's Exhibit 103, Dewar and Redwood, 1890, "high pressure."

Petitioner's Exhibit 107, Redwood of 1896, "high pressure."

Petitioner's Exhibit 120, Intschick, 1890, "pressure."

Petitioner's Exhibit 129, Schuchow & Gavrilow, 1891.

Petitioner's Exhibit 133, Pielsticker, 1891.

Petitioner's Exhibit 134, Pielsticker, 1892.

Petitioner's Exhibit 104, Laing, 1892, 80 pounds.

Petitioner's Exhibit 105, Laing, 1892, 10 pounds.

Petitioner's Exhibit 143, Mann, 1899, 50 pounds.

Petitioner's Exhibit 102, Kraemer and Spilker, 1900, 25 to 10 atmospheres.

Petitioner's Exhibit 136, Barbet, 1903, 5 to 6 atmospheres.

Petitioner's Exhibit 141, Barbet, 1906, 5 to 6 atmospheres.

Petitioner's Exhibit 154, Nikiforoff, process, 1896, 2 atmospheres.

Petitioner's Exhibit 149, Nikiforoff, 1904, 15 pounds or more.

Petitioner's Exhibit 148, Testellin & Renard, 1908, 5 atmospheres.

Petitioner's Exhibit 131, Kittle, 1910, 6 atmospheres.

Petitioner's Exhibit 135, Laing, 1910, "pressure."

Petitioner's Exhibit 132, Turner, 1912, 60 pounds.

Petitioner's Exhibit 144, Graeffe and Walther, 1913, 20 to 50 atmospheres.

Petitioner's Exhibit 128, Greenstreet, 1914.

Mr. Rauber: What was the date of your question?

[fol. 704] Mr. Wiles: He said he had seventeen, and I just wanted them listed, that is all.

Mr. Rauber: Did you specify any date?

Mr. Wiles: I would just as soon he would go on and give all he has.

A. (continued). "Petitioner's Exhibit 122," Bacon, Brooks & Clark, 1915, 60 to 300 pounds.

"Petitioner's Exhibit 152," Edwards of 1916, "High Pressure."

These last ones are, of course, not prior art. There are several earlier ones.

Mr. Rauber: You mean prior to Burton?

The Witness: Prior to Burton.

A. (continued). "Petitioner's Exhibit 93," Young, 1865, 20 pounds.

"Petitioner's Exhibit 94," Thorpe and Young, 1871, pressure not given but "Under Pressure."

"Petitioner's Exhibit 95," Thorpe and Young of 1872, speaks of it as "Under Pressure," but I do not recall if they give the pounds pressure.

"Petitioner's Exhibit 96," Thorpe and Young of 1873, 20 to 25 pounds.

"Petitioner's Exhibit 97," Riebeck of 1886, two to six atmospheres.

I believe that is a total of 32 citations under pressure.

Q. You referred to the statement of Dr. Humphreys in the affidavit in the file wrapper of the Burton patent, which affidavit shows a 20 per cent absorption of sulphuric acid in Burton distillate. In your judgment is that a substantially correct statement of the sulphuric acid absorption?

A. Which number is that?

Q. That is the original.

A. '667?

Q. 1,049,667.

The Master: That is a product patent?

Mr. Wiles: No, the original process patent.

Mr. Wiles: Now, check that 20 per cent. That is from recollection, Doctor; I think it is 20.4 to be quite accurate.

The Master: Of that distillate?

Mr. Wiles: That type of distillate.

A. I cannot tell from reading the statement here as to what kind of materials he worked upon, whether upon one [fol. 705] which was the crude gasoline obtained, or whether it was upon a product which had been previously treated with sulphuric acid. He makes some statements here in that paragraph which are odd for one who is occupying a position of chief chemist. He speaks of it as 95 per cent sulphuric acid. I presume he was referring to 66 degree, pure acid, which would be 93 per cent and a fraction.

The Master: That is enough; he says he can not tell.

The Witness: The percentage he gave is 20.2.

Q. In the process of the Laing patent for a multiple compartment still, operating in accordance with the specific example of the patent, being a 70 degree distillate—what will be the contents of the last compartment of the still?

The Master: Do you mean in a general way?

Mr. Wiles: Yes, your Honor.

The Master: And assuming that you are working on crude stock?

Mr. Wiles: On the stock that Laing gives. Laing names his stock. He names his pressure and product, and in his paper he gives approximately the temperature—that is, he [fol. 706] gives the temperature at atmospheric, and I presume it would have to be correspondingly higher for——

The Master: Is that a publication, or a patent?

Mr. Wiles: Both—he has two patents and a paper before the Royal Society, also.

The Master: Can't you designate by number, what patent you refer to?

Mr. Wiles: What I mean, there, is the process of the——

The Master: Of the paper?

Mr. Wiles: He has one patent on a pressure distillation process, and another patent, in which he shows a compartment still. He says, in the pressure process patent, that this may be applied to the compartment still patent, and the thing we have always considered, at least, and that I have most thought of, is the Laing process operating under pressure with the compartment still as described in the two patents, when read together, and the witness also discussed that same matter, and the combination was also discussed in the paper.

The Master: When you refer to a still with two compartments, is that thing that is shown the equivalent of a still with a re-action chamber, or a separate reaction tank?

Mr. Wiles: I don't think so, but I am leaving that up to the witness, of course.

The Master: You might make reference to the particular number of the patent, so that there may be no mistake about it—the patent that shows that compartment still, together with the publications, and the other patent.

A. The Laing patent, No. 471,291, of 1892, which gives the 70 degree distillate—or, rather, he gives it as .700 specific gravity?

Q. That is the same, isn't it?

A. Approximately the same.

Q. The curves cross at 70?

A. Approximately. He does not state as to what the temperature of the last compartment would be. I do find, however, in the Laing citation, Transactions of Royal

Scottish Society, of 1894, a statement that "For instance, if you wish an oil whose boiling point is 250 degrees Fahrenheit, you must have your last compartment kept steady at that temperature, and no oil can possibly come over, whose boiling point is higher."

And he gives a method of regulating the temperature of this compartment. I do not recall what the end point is, [fol. 707] for the 70 degree naphtha, but we would expect, by combining these two references of Laing, to use a temperature which would be approximately the highest temperature to which a sample of 70 degree Baume product, would reach, in distilling.

Q. It would be around 250, or somewhere in that order, wouldn't it?

A. Yes; I think it is a little higher—probably around 300.

Q. All right—take 300 as the figure, and then go ahead.

A. I think that answers the question as to the temperature.

Q. I know, but what will be the contents of the last compartment of the still?

A. The contents of this still compartment, would be, to a considerable extent, light boiling gasoline, mixed with heavy residues from the earlier compartments—that is, the residues would pass down through the opening between the diaphragms, from compartment to compartment—the light products distill through the columns, L2 and L3, and condensers, M2, and M3, and into the last compartment, B4, and then pass out through the goose-neck, N—so that the composition of the product, in the last compartment, would be a mixture of quite light material, with quite heavy material, and only a small amount of intermediate material.

Q. And how will that compare with the 97 specific gravity, or 14.3, Baume, charging stock, that Laing employs? Would it be lighter, or heavier?

A. It would probably be lighter. The exact gravity would depend upon the proportion between the two mixtures.

Q. What will be the direction of flow of liquid under the last partition of that still?

A. To the right.

Q. That is, towards the last compartment?

A. Yes.

Q. And in making that statement you have clearly in mind that it is making 70 gravity distillate?

A. Yes and that the 70 gravity distillate is rapidly taken out through the goose-neck, N.

Q. Now, taking Humphrey's run-back patent, with the dismensions given by Humphreys, an 8 by 20 still, two one foot by forty foot pipes, for run-back, and assuming the still to be running in accordance with the preferred conditions given—that is, in accordance with the Burton pro- [fol. 708] cess for the manufacture of gasoline—or, essentially, gasoline—of the total vapors leaving the still, what proportion will be condensed by the run-back and returned to the still?

A. It cannot be told, because there would be a variation; for example, with a cold day like this, with much wind, there would be much condensation, and with a hot day, and no wind, there would be little condensation. It cannot be told, and it would depend, to a considerable extent, of course, on the question of the speed of heating, which Humphreys does not disclose.

The Master: Returning to that Laing disclosure—assuming that the operator desired a larger amount of low boiling point fractionation, if he was seeking to separate gasoline, instead of kerosene, would he know how to regulate his temperatures and pressures to take off those boiling point fractions, with that apparatus, and in the manner disclosed by Laing?

The Witness: Yes, because, in fact, in this particular case which has been under question, he is making a gasoline, and the temperatures given, are such as would exactly fit in the case of gasoline, and would have to be changed, if he was making kerosene.

Q. Now, referring, again, to the Humphreys' patent—assuming that on a summer day, and with no wind, with 80 degree atmospheric temperature, and making three per cent per hour of distillate, what proportion of the vapors leaving the still would be condensed and returned to the still?

The Master: What Humphreys' patent do you refer to?

Mr. Wiles: No. 1,119,700.

A. As I find it, neither Burton, nor Humphreys, give the question of a three per cent per hour distillate.

Q. I asked you to assume it.

A. Oh, you are giving that, are you?

Q. I asked you assume it.

A. It is, again, I might say, impossible to calculate this. It would depend upon the oil, in addition to the temperature. It would depend upon the thickness of the pipe, and it would depend upon many other factors, and it would depend upon the exact pressure used, so that the temperature—in other words, the speed of cracking, and the speed of cracking would depend upon, as I have intimated, the oil, as well as the temperature. So that it is a question, which is, of course, impossible to answer.

Q. I am perfectly willing for you to make any assump- [fol. 709] tions that you ask for, to tell those figures. In the first place, isn't that a chemical engineer's question, that can be calculated?

Mr. Wiles: Well, I don't care about that.

A. I don't think it is, because you wouldn't have anywhere near all the data there. That is, we haven't the rate of reaction of our stock, we haven't how long the plant has been under operation—whether it is the beginning or the end of the operation. It is foolishness to try to make a calculation of that kind.

Q. I am perfectly willing for you to make any assumption that you want to, and use any data you want to. You say the thickness of the pipe would make a difference. How much difference would that make?

A. It would depend upon the thickness of the walls, as to how much difference it would make. But there are other phases in there. Even if we did know the thickness of the walls of the pipe, the major factors would still be unknown, and I don't believe anybody, taking the Humphreys patent, and the Humphreys process, and the Burton process, who has only made an arbitrary measurement of it, can answer your question, because, in such a case as this, the question of actual practice is approximately the only way to find out.

Q. Well, taking the pipe, which you said was one of the factors, and assuming that the pipe used, is either single or double strength, twelve inch, which is either a quarter or a half inch thick—how much difference will that make—what percentage of difference?

A. You say single strength is a quarter inch, and double strength is half?

Q. Yes, in a twelve inch pipe?

The Master: I suppose you mean, by your answer, that an actual trial would demonstrate a positive result, for the reason that— as I assume you mean—given any such apparatus, or any apparatus that is constructed, temperatures will vary from hour to hour, and pressures will vary from hour to hour, and conditions might give you, on a two or three hour run, at one time, a result differing from the results obtained on a run of a similar length at another time?

The Witness: That is correct. And even Humphreys, himself, suggests here—he cuts off one of the pipes, sometimes, at the last of the run.

[fol. 710] The Master: And there is always a range of variation in all those operations—is that what you mean?

The Witness: That is true, and the variation, in this particular case, is extreme.

Q. Now, you have said that the pressure will vary from time to time—in answer to the Master's question—isn't it a fact that the pressure can be, and usually is, in practical operations, held within two pounds, over twenty-four hours operation?

A. The pressure will not, probably, vary greatly, but the rate at which the material is distilled, will vary greatly—just as Humphreys, himself, says:

"When the end of the run approaches, and *and* the flow of the vapor is slow, one-half of the conduit can be closed, so that condensation can be maintained, of the proper amount."

I presume he means the proper ratio.

The Master: Do you mean to say that from the initiation of the operation, assuming that you are putting fresh stock in your apparatus, that the pressure is constant, all the way up, and that there is no time when you have to build up your pressure to the pressure desired?

Mr. Wiles: In the Burton operation, as conducted, practically, we do not vary two pounds a month, as a rule.

The Master: You mean, once after you have got your apparatus going, and have developed your pressure?

Mr. Wiles: As shown in the Clark patent, we bring up the pressure while it is cool, and it is brought up to the pressure of all the other stills.

The Master: Assuming that that is a fact in the art, it has to be applied, in your questions, to the Humphreys patent. I suppose Humphrey- also had to develop his pressure.

Mr. Wiles: I am willing to have the characteristics assumed—as many as the witness wants. He may assume that the Humphreys still has gotten up to 80 pounds, and is running at 80 pounds, and stays within two pounds of 80 pounds, throughout the run—if that will make any difference. He may assume that the pipe is a 12 inch double strength pipe, with a half inch wall. He may assume that the vapors, if he chooses, enter the bottom at 700, and I have told him the distillation rate, of three per cent per hour, and I have given him the weather conditions and the external temperature.

The Master: And you want to know how much vapor will be condensed?

[fol. 711] Mr. Wiles: Yes, in the run-back. It is a perfectly straight engineering calculation, and if there is any other data he wants, I will try to give it to him.

The Master: Do you think you can answer the question?

The Witness: I don't think anybody can answer that with any degree of accuracy.

Q. What is your approximation?

A. I think it would be foolish for anyone to try to approximate that, with the thought that they could approximate it with any degree of accuracy, except to compare it with a still of this kind, which had been running previously.

Q. Could you tell whether there would be two per cent condensed, and returned, or 98 per cent, or anywhere between?

A. I have answered your question, in answering your second question before this one.

Q. Well, are you so far apart that you couldn't state between two per cent and ninety-eight per cent of the total?

A. Of course it would not be ninety-eight per cent of the total that would be there, but the amount it is impossible to measure, or estimate within any accuracy, worth while, on a calculation of this kind.

Q. You say "of course it would be not ninety-eight per cent"—can you say, equally, that it would not be eighty per cent, or ninety per cent, or seventy per cent?

The Master: The best evidence, it seems to me, would be to get someone who has ever run the Humphreys still, and get his record and find out.

Mr. Wiles: Your Honor, this is most illuminating of the witness' knowledge, when he says that the Humphreys run-back is just the same as the prior art. I want to know what he knows about what the Humphreys run-back does. He says that it is just the same as something else in his operation and effect.

The Witness: I didn't say it was the same in quantity, because the stills are different in size, and the other conditions are different.

Q. But in proportion to the output, does it do the same thing, exactly? If not, how far is it from the art?

A. It is essentially the same disclosure, and there is no use of quibbling as to the exact percentage.

Q. Now, can you tell, in the prior art which you have stated, what proportion of the vapors anyone of them condenses?

[fol. 712] A. Just as in the case of Humphreys, the proportions could be changed by the change of weather—if it was air exposed, as here—or by other schemes.

Q. Then you cannot tell anything about any of them, between two and ninety-eight per cent?

A. I have said that I can, and I have stated that it is much less than ninety-eight per cent—probably only a few per cent, but as to the exact amount, it cannot be calculated readily, with any accuracy.

Q. When you say "probably only a few per cent," do you mean around ten per cent returned, or probably something of that sort? I just want to get an approximation.

A. I don't want to give a figure on that, because to give a figure would be misleading.

Q. Why misleading? It would tell us what is in your mind.

A. But I would judge that a man like you might even quote that figure, as saying that I said that that was what it was.

Q. I want your best judgment as to what it is, I want the theory, and what has been in your mind, when you have

been testifying here as to all of these devices, when you say that it returns and refluxes—what kind of a percentage have you had in your mind all this time?

A. A sufficient percentage to permit the getting of a distillate which would be permitted to be used for gasoline.

Q. What is that? Two per cent, five per cent, ten per cent, sixty per cent—or what?

Give us some sort of an idea, what you are talking about.

A. The amount certainly would be less than ten per cent of the total product, in the period of an hour.

Q. The amount returned?

A. The amount returned—whether it would be more than one per cent—it might be as even as low as one per cent. It would depend a good deal on the question, for example, upon the insullation of the top of your still. It would depend upon the time of your operation—whether it was the beginning, in the middle, or at the end—and it would depend upon many, many other factors—the question as to the kind of stock used, the condition of that stock, and its previous treatment and other things.

Q. Varying somewhere from one to ten per cent, so far as you now know?

Mr. Rauber: What do you mean?

[fol. 713] Mr. Wiles: The return. He said it would certainly be less than ten per cent.

Mr. Rauber: Your Honor, I think that is a rather unfair question. It all depends on what kind of stock you are using.

The Master: Let the Doctor go ahead and handle this the best way that he is able to handle it. He is able to take care of himself.

Q. Now, when you said it certainly would be less than ten per cent, did you mean that the amount returned in an hour would certainly be less than ten per cent of the total vapors leaving the still, in that hour?

A. No.

Q. What did you mean, then?

A. I said, as you are measuring the vapors leaving the still, per hour, as three per cent of the total stock fed, so I am basing this, also, on three per cent of the total stock, fed.

Q. Oh, well—what I said was that we were making three per cent of distillate over, in the final condensing.

A. I understood that.

Q. Now, that is three per cent of the contents of the still.

A. Yes.

Q. And the still not being fed, at all—just a batch still—taking off three per cent of its contents in an hour—that is, taking about a—well, whatever it is—three per cent of the contents of the still, eight by twenty. Now, when that condition is prevailing, and three per cent of the total contents of the still is leaving the run-back, and being condensed in the final condenser, what proportion, approximately—what is your best estimate, of the ratio between the amount returned and the amount that gets over into the final condenser?

A. That is quite a different question from what you asked before.

Q. Well, it comes to the same thing—it comes to different figures. What proportion—I will put it—of the total vapors which depart from the still into the run-back, are returned?

A. I have said that I cannot tell, on that, and I cannot answer that, because it would depend upon the stock used, the temperature fired, the amount of insulation on the top of the still, whether it was insulated, at all, or not, and, many, many other factors—and, as I have said, I cannot tell.

Q. You said, somewhere, that it would be certainly less than ten per cent. What did you mean by that?

[fol. 714] A. You were basing, before, and suggesting that it would be between one and ninety-eight per cent of the stock in the still.

Q. Oh, no, no—I meant of the total vapors, all the time, I have never asked a question about the stock, excepting to base the total distillate on the stock, as figuring the running rate of the still.

A. That was the only percentage that you mentioned.

Q. Exactly, but I always asked you what proportion of the total vapors leaving the still, would be condensed.

A. I don't think you used that term, at all, before—what proportion—excepting just lately.

Q. That is all I have asked you about. Well, if you don't understand——

The Master: Well, can you answer that question?

The Witness: I told him I could not, with any accuracy.

Q. I want to know, not with any accuracy——

A. Well, I can't answer it without knowing the other things about it, and without having something to compare it with.

Q. Now, what is it you want to know, and give me all the facts, one at a time, and I will give you your hypothesis.

A. In a small Humphreys still——

Q. Eight by twenty.

A. No—wait a minute—I want to know, for comparison, in a small Humphreys still, working on a much smaller charge than here, what the run-back is.

Q. I am not going to give you such data as that.

A. Yet that would be necessary, practically, in order to do that, because it would be practically impossible——

The Master: Are you trying to match your records of actual operations against his opinion of hypothetical operations? I don't think that is fair, at all.

Mr. Wiles: No, I am not, your Honor. I am trying to find out what he has had in his mind, when he says that the prior art and Humphreys are alike. I want to know what he thinks Humphreys really is. He says that it is the same as something else, and yet he has not told us what either of them is, and I want to know what he thinks the real function of this Humphreys run-back is.

The Witness: I can answer your last statement, there, very easily, and that is, that in the case of the prior art, you can, as in the case of Humphreys, change it so that it will run back a sufficient amount, even if it doesn't run [fol. 715] back the right amount, at first—either by additional cooling, or by additional size, which would give additional cooling, or by decreasing it, if it is too much. So that we can get our run-back effect from these others—by the amount of cooling.

Q. There is no question but that a mechanical engineer can design to do anything he wants to do, after somebody tells him what to do, is there?

A. It has been found, for example, with another type of run-back a column still, that the theoretic phase will not serve, and that they have to make, and do make their calculations by making comparisons with the known operations of column stills of certain size. That is one type of

run-back, and this is another type of run-back, and it is no easier to calculate this one than it is the other one. We can calculate each of them, but we know that our errors are in the order of four hundred per cent—or, maybe, as much as four hundred per cent.

Q. And in the case of prior art, there are different sizes and different shapes and designs, also, aren't there?

A. Yes, there are.

Q. And, therefore, they might vary four hundred per cent?

A. From the theoretical—in some cases, but not in all cases would they vary four hundred per cent.

[fol. 716] Q. Have you, Doctor, the fractionation of the shale oil gasoline, that I asked you for?

A. Yes. The starting point—the first portion came over at 212 degrees Fahrenheit; fifty per cent at 300 degrees Fahrenheit; ninety per cent at 370 degrees Fahrenheit, and finished at 430 degrees Fahrenheit.

Q. Is that Engler distillation, Bureau of Mines method?

A. Yes, on that. I have also a second shale gasoline sample, which is one subject to another refining, which started at 200 degrees Fahrenheit, and the end point was somewhat lower—some 360 degrees Fahrenheit.

Q. And that is, likewise, by the Bureau of Mines method?

A. Yes. I don't happen to have the percentage.

Mr. Wiles: I am through.

Cross-examination.

Questions by Mr. Dorr:

Q. Now, Professor, I believe our Company has had the pleasure of having you visit its refinery at Providence. Will you fix, as accurately as you can, the date of that visit?

A. It was a visit at the time of the meeting of the American Institute of Chemical Engineers in Providence, either the last few days of June, or the first of July.

Q. Did you make the visit with the Society, or individually?

A. I left early to catch a boat to New York, and I made the visit with the Society.

Q. And that was in 1925?

A. Yes, before my connection with this case.

Q. And that visit is the source of the information that you have given us, as to the Providence refinery?

A. Yes, that is correct.

Q. And who went around the refinery with you?

A. I don't know the name of the individual. It was someone who had technical training there. I did talk to one of the still operators, also.

Q. Can you describe the individual in any way, so as to enable us to identify him, particularly?

[fol. 717] A. I doubt if I can. I am not good on such descriptions, and I paid no particular attention, because I was more interested in showing my boy, of nearly fifteen, the question of how a big plant operates, than I was in getting details regarding the operation.

Q. This was before you were retained as an expert in this matter?

A. Yes, sir—six weeks before.

Q. And you did not have this matter under negotiation at that time?

A. No. I knew that there was such a case and I had been approached, and I told the Government, at the time, that I could not very well serve, and suggested certain other people, and it had passed out of my mind at the time.

Mr. Dorr: That is all.

Mr. Cooper: I will cross-examine this witness on behalf of The Texas Company.

Cross-examination.

Questions by Mr. Cooper:

Q. You have spoken, in answer to one of Mr. Wiles' cross questions, this morning, to the effect, as I understood it, that you were afraid you would be quoted if you said anything specific in answer to his question, as to the results, in one of the patented processes—the Humphreys patent, I think. Do you understand that you are a paid and sworn advocate for the Petitioner in this suit?

A. No. I am paid, but I am not particularly an advocate for it. But if you will remember what I said to Mr. Wiles, it was a question of misquoting, rather than quoting.

Q. You are, yourself, a designer and operator of plants for cracking, are you not?

A. I have never been an operator of such plants.

Q. Didn't you swear that you were vice-president of a company, in charge of a cracking plant?

A. I have been in charge of the operations, but not as a workman. That is, not an operator.

Q. Well, didn't you know the facts that he inquired about?

A. Oh, yes.

Q. And you didn't want to give them because you were afraid you would be misquoted—is that it?

[fol. 718] A. Well, I was afraid, from the way the questions were framed, that my answer might be misquoted. But, on the other hand, I didn't want to give them, because they could not be given accurately.

Q. Well, if you give facts how do you suppose you could be misquoted? You were asked for facts.

A. I was not asked for the facts, but I was asked for an estimate.

Q. You were asked for what are known as scientific facts—your judgment, and opinion, based upon your inventions, as a designer and operator of a plant for cracking, were you not?

A. I was not. I was asked for an estimate. That is not a scientific fact.

Q. Aren't you asked for estimates, in the regular course of your business?

A. Yes, we carry out experimental work and tests, in order to give those estimates.

Q. Why shouldn't you give estimates here, of a scientific nature, when you are asked for them?

A. I would be perfectly willing to carry out the test and do as we do in consulting work—carry out tests, in order that we may give estimates. But we do not give estimates which have so little basis of fact, as the kind of an estimate that I was being asked for.

Q. I don't yet understand why you should not give scientific facts, because you are afraid you would be misquoted. What difference does it make to you whether you are quoted, or misquoted, in this case?

A. I am not afraid to give scientific facts, but I do dislike to give guesses—and that was what I was asked to do.

Q. What difference does it make to you whether you are quoted, or misquoted, in this case?

A. No man likes to be misquoted.

Q. At some place in your cross-examination, you brought in, in an answer, a reference to the Rittman process. Tell me what you meant by that—not in any exhaustive way, but so that we will know what you had in mind, there.

A. The Rittman process is a vapor phase process, but [fol. 719] operating under pressure, in an apparatus which was installed in some plants for the making of benzol, or toluol, and also, at other times, for the making of gasoline.

Q. Rittman was a man in the Government employ, was he not—in the Interior Department?

A. When he worked out this process?

Q. Yes.

A. No, he was a student in Columbia University, in the department, where I am a professor, and, later on, went to the Government, and did work for the Government.

Q. Were you there when he was in Columbia?

A. No.

Q. He went into the Government employ and worked out his process, and it was extensively exploited by the Government, and then was dedicated to the public, was it not?

A. I don't know.

Q. If that is a fact, you don't know it?

A. I don't know about it being dedicated to the public, at all.

Q. Where do you say it was in actual commercial use?

A. I said it was installed. I didn't say it was in actual commercial use.

Q. Where was it installed?

A. I think, as I recall it—and this is only a memory of eight or nine years ago—it was installed by the Aetna Powder Company, in a plant in Pennsylvania, for the making of benzol, and possibly, toluol.

Q. Then what became of it?

A. That particular plant?

Q. Yes.

A. It was shut down, either about the time the War ceased, or, possibly, a little before.

Q. Now, isn't it a fact, within your knowledge, in this art, that the Rittman process was dedicated to the public?

A. It is not. I don't recall that. In fact, the statement that you have made just now, or suggestion, is the first

that I think I ever heard of it being dedicated to the public.

The Master: In what sense do you mean dedicated to the public, Mr. Cooper?

Mr. Cooper: I can bring that out by another question.

The Master: Do you mean it was a non-patented process?

Mr. Cooper: As I understand—and this is only a matter of information, to me—it was a patented process, and was exploited by the Wilson Administration, and largely ex-
[fol. 720] ploited in the technical and lay press, in the period from 1912 to 1915, as something which would produce cracked gasoline in large quantities, and that it was available to anybody in the United States to use, because there was a dedication of the invention, patented, or unpatented, to the public—that it was free for use by the public.

Q. Now, you may remember that you testified quite extensively, in the early part of your direct examination, here, about the Adams patent, No. 1,327,263, and about its application, which, I believe, was serial No. 618,011, and about the contents of that application—do you recall that?

A. I remember that this patent was under discussion. I don't remember just exactly what was said. Can you give me a reference to the page?

Q. I am asking you whether you recall having testified about that patent, and about the contents of its application, serial No. 618,011.

A. Yes, I recall the discussion in connection with the file wrapper.

Q. And you observe, do you not, that that application was filed on March 30, 1911?

A. Yes.

Q. Did you have your attention called to the fact that there was an earlier application of Adams, known as application, serial No. 535,879, which was filed in December, 1909?

Mr. Rauber: What is the number of that, Mr. Cooper?

Mr. Cooper: No. 535,879, which was filed in December, 1909?

A. I have here a copy of the file wrapper which so states.

Q. That is, you have there before you, just handed to

you by Mr. Rauber, an application, serial No. 535,879, filed December 31st, 1909?

A. No.

Q. Then it is not the same?

A. No, this is not that application; it is another one.

[fol. 721] Q. Very well. Now, go back to the question I put to you: Did you have called to your attention an application of Adams, serial No. 535,879, filed on December 31st, 1909, for an improvement in oil converting process?

The Master: Did it result in a patent?

Mr. Cooper: No. I will come, in a moment, to just what it did result in.

A. I do not recall that I had such a file wrapper, or patent application called to my attention.

Q. Now, assuming for the purpose of this question, that that application, serial No. 535,879, was filed December 31st, 1909, and that it made substantially the same disclosure as the application No. 618,011, that matter was not brought to your attention in connection with your preparation for this testimony?

A. I do not think it was.

Q. Of course, then, it was not brought to your attention, that that earlier application, No. 535,879, was held to have been abandoned, as an application by the Patent Office on March 14th, 1911?

A. I have no such recollection.

Q. Did you find in the file wrapper, known as application No. 618,011, any reference to this prior application, No. 535,879?

A. I do not recall that I did.

Q. Now, refer, for a moment, while I am having something else looked up, to Adams patent, No. 1,320,354. Did I understand you to say when you were discussing that patent that you had examined its file wrapper, being the proceedings under application filed December 1st, 1909, serial No. 530,852?

A. Yes, I read parts of that file wrapper.

Q. And that as a result of your examination of the file wrapper, I understood you to testify that the applicant, when he filed that application, did not disclose a pressure operation but only a vacuum operation?

A. Can you give me the page of that testimony?

Q. I haven't any pages marked.

A. I read from page number 16——.

Q. No, no. I am asking you what you testified to, not what you read from some paper.

A. I testified to that because the Examiner's letter called attention that the apparatus is for carrying out the process disclosed and claimed in patent No. 976,975, and he further states:

"A perusal of such patent shows that it is concerned with vacuum distillation, and that special means are provided therein for the production of a partial vacuum in the still."

Consequently, it is concerned with a vacuum process. It is an apparatus patent, which, from here, is evident that it is for the use of a process using a vacuum.

[fol. 722] "There were, to be sure"—(I am reading from paper No. 16):

"There were, to be sure, some hints about having pressure in the still. It is significant that in the meantime others had pointed out that it does make considerable difference whether a valve used to develop pressure in a still is located between the still and the condenser, or beyond the condenser. It thus becomes of great importance in this case to ascertain whether there is any basis for the present claims, which claim specifically a pressure producing valve beyond the condenser."

This, of course, was paper No. 16, very well along in the case, and is of date of 1916, March 22nd.

Q. You think you have answered my question?

A. I do.

Q. You understand the English language?

A. Reasonably well, sir.

Q. I will repeat my question: Is it your testimony that when this application 530,852 was filed, which eventuated in the patent No. 1,320,354, it did not disclose a pressure operation?

A. No, it is my present testimony—my testimony was that vacuum and pressure were equally important. I refer to page 333 of the typewritten statement.

Furthermore, the original application gives them as essentially equally important.

In the original application he refers to the pending application:

"That this relates to an apparatus having a capacity of serving as a means instrumental in carrying out the oil converting process disclosed and claimed in my co-pending application No. 417,713, filed February 25th, 1909 (They have it)."

They have afterwards changed to the patent as it is issued, and that is the patent which is the vacuum patent?

Later on in the application he does mention pressure, as I said here in my original testimony which you disquoted, that pressure and vacuum were equally important. On page 20 of the original specification, line 16, is, I believe the first place that he refers to pressure.

Q. Did you not testify on your direct examination—(I am reading from the top of page 409 of the printed record)—with reference to this application now under consideration:

"In the original specification in the drawing there is no pressure valve indicated either between the condenser and the retort or after the condenser."

Did you or did you not so testify?

The Master: 409 of the printed record?

[fol. 723] Mr. Cooper: Yes, right at the top.

Q. I am asking you, did you or did you not testify to this (handing document to witness)?

A. I so testified.

Q. I now read as follows from the application as originally filed:

"In case it is desired to act upon the oil under heavy pressure; valve 146 and 147 will be closed to cut out the automatic float, E, and the pump, F, will be put into operation to draw oil from the reservoir, B, and force the same under heavy pressure through the preheater, D. Dependent upon the adjustment of the valves, the oil may be caused to flow either upwardly or downwardly past the electric heater, 51.

In this connection, it is to be noted that the pop valves, 124 and 143 not only control the pressures, but also act as safety valves, for when the pressures become excessive, one

or the other or both of these valves will open and permit the vapor and fluid oil to escape into the return parts of the system."

A. This is on page 20 of the original specification and is what I referred to in my previous answer, in which I referred to page 20, line 16, which is just what you have read.

Q. Are not those two valves, 124 and 143, pressure valves between the condenser and the retort?

A. No, and they are not those numbers at all.

Q. Why not? Look at Fig. 2.

A. The valves are 146 and 147 in Fig. 2.

Q. On my copy they are 124 and 143.

A. But they are not in the original specification.

Q. Yes.

A. (continued). They are 146 and 147, which are adjacent to the pump feed.

Q. You are looking at the wrong valve.

A. Wrong what?

Q. They are way over by the——

A. There they are (indicating).

Q. Here (indicating), 143 and 124, between the——

A. The valves, 146 and 147——

Q. I did not say anything about the valves 146 and 147; I say the valves 124 and 143.

A. Oh, those are the safety valves, or pop valves, rather.

Q. Yes. Are they not pressure valves between the condenser and the retort?

[fol. 724] The Master: "And serve to maintain pressure on the retort, but not on the condenser?"

Mr. Cooper: Yes. What the specification says about them is, "In this connection it is to be noted that the pop valves, 124 and 143——

The Master: "Not only control the pressure——"

Mr. Cooper: "Not only control the pressure but also act as safety valves."

Now, my present question——

The Master: That is, they control the pressure on the retort.

Mr. Cooper: Yes.

Q. Now, my question is: Are thye not, those valves, 124 and 143, pressure valves between the condenser and the re-

tort, and does not the specification, as originally filed, so state?

The Master: Answer yes or no.

A. Yes, but they are for the return—"to permit the vapor and fluid oil to escape into the return parts of the system." It states just below, "valve, 134, being so positioned and placed as to maintain the level in the receptacle, E, in the position desired."

Q. How about 124?

A. 124?

Q. Yes.

A. It states: "For the permitting of the vapor and fluid oil to escape into the return parts of the system."

Q. Where does it say that?

A. Lines 26 and 27, page 20.

Q. Yes. And it also says:

"In this connection it is to be noted that the pop valves, 124 and 143, not only control the pressures——"

What do you think he meant by that?

A. I imagine that here he is dealing with the question of operating his process under pressure, as well as in the early portion under vacuum.

Q. Now, read all that you have from the early part of the specification that describes an operation under vacuum?

A. I read first from page 2, line 1, or beginning line 1:

"This invention relates to apparatus having capacity as serving as a means instrumental in carrying out the oil converting process, disclosed and claimed under my co-pending application, No. 417,713, filed February 25th, 1909."

Now, it is marked as issued into patent No. 976,975, [fol. 725] granted November 29th, 1910. And later, it is called attention that: "A perusal of such patent—" I refer to paper No. 16 of March 22nd, 1916.

Q. I am talking about the original specification.

A. (continued). Shows that it is concerned with vacuum distillation and with special means——

Mr. Cooper: Excuse me, your Honor. I want a ruling on this.

The Master: Wait a moment. The question called for the original specification, not for the file wrapper.

The Witness: This is a clear reference to a vacuum scheme.

Q. Now, you have answered my question fully, have you, as to——

Mr. Cooper: Just read it again so we will be sure he has answered it all.

The Master: What is the distinction for an apparatus operated by gravity, with the oil flowing either upwardly or downwardly past the electric heater, or an apparatus operated under high pressure with a corresponding alternativeness in the flow of the oil?

The Witness: The first one might be under vacuum, or at atmospheric pressure; the second one would have to be of superatmospheric pressure.

The Master: You mean, in connection with his reference to his prior patent, which was for a vacuum process——

The Witness: This being an apparatus patent.

The Master: And this being an apparatus patent, you think that was put in there to cover an operation—that reference to the apparatus to be operated by gravity, was to cover an operation under gravity?

The Witness: Yes.

The Master: In the alternative.

Q. Do you still think that your statement on direct examination, which I read from the top of page 409:

"In the original specification in the drawing there is no pressure valve indicated either between the condenser and the retort, or after the condenser" is correct?

A. I have not considered this pop valve as a valve for controlling the pressure. It might be so considered, and if it is so considered, then my original statement would have to be modified.

[fol. 726] The Witness: Yes.

Q. And the specification so states specifically, does it not, as originally filed, where it says:

"In this connection, it is to be noted that the pop valves, 124 and 143, not only control the pressures, but—" and so forth?

A. It could also act as safety valves, so that they do do that when using the alternative method of pressure.

Q. Have you had your attention called to the fact that in the original claim as filed, that this specification, to-wit, for example, claim 50, pressure is specifically——

A. Claim what?

Q. Claim 50.

The Master: Original claim 50.

Q. (continued). Claim 50 refers to the pressure as follows:

"An apparatus of the nature disclosed comprising in combination a series of holes, a receptacle connected therewith, means for forcing the oil from said series into said receptacle under pressure, and means for subjecting the oil in said receptacle, while under pressure, to the action of a member maintained at incandescent temperature."

A. I think I saw this claim. I do not recall that my attention was called specifically to it, but it simply fits in with the general idea that the application covers both vacuum and pressure.

Q. Now, I call your attention specifically to the fact that so far as my reading goes, none of the original claims as filed with the application cover a vacuum operation at all; is that a new thought to you?

The Master: That is, none of the claims refer to operation by gravity?

Mr. Cooper: Not as far as I can see.

A. The claims, primarily, are a description of apparatus, and it is only incidentally in a few claims that process is mentioned.

Q. What do you mean by "incidentally" and "in a few claims"? How do you know how many claims refer to pressure?

A. Well, I went through this. There are a total of 54 claims.

Q. What is incidental about the reference to the pressure in claim 50, which I just read to you? What do you say is incidental about that reference to pressure?

[fol. 727] A. Because they are mentioning here a process incidental to the mentioning of an apparatus—to describe an apparatus, and it simply has a means, and while they do not say "a pump," they could have used, perhaps, "a pres-

sure pump," which would have shown pressure, as well, but in general there is no reference to the question of pressure in the other claims.

Q. Do you say that Adams did not disclose a pressure pump for forcing the oil in?

A. No, I did not say that, but I say that in the claim, it is only incidentally that—I will put it this way: That in a claim for apparatus, it is only——

The Master: Where does he disclose the pressure feed pump?

Mr. Cooper: He discloses it in the drawings, your Honor.

The Master: I do not find it. I find pressure, I find a pump for returning the oil, the pump, R.

Mr. Cooper: Let me read the specification again.

The Master: Can you not refer to a drawing where there is a feed pump on here?

Mr. Cooper: The pump, S, "will be put into operation to draw the oil from the reservoir and force the same under heavy pressure through the preheater, B."

Q. Do you say, Doctor, that that is incidental, too?

A. No, I said that the patent discloses equally well pressure and vacuum—the application discloses equally well operation under pressure or vacuum. The patent application is for an apparatus, and, accordingly, we would not expect to have, in the claims, discussions of process excepting as they might be incidentally put in, and we happen to have in one of 54 claims here, a mention of the use of pressure.

The Master: It mentions the functions of the different parts?

The Witness: Yes, and it is simply there because it happens to show a pressure.

Q. Now, just refer by letters of reference to those parts or elements shown in the drawings which create the vacuum.

A. In Fig. 1 I do not find any indication of either pressure or vacuum; in Fig. 2 I find a feed pump concerned; and safety valves or pop valves of safety valve type, 124 and 143, which may be operated at either atmospheric pressure, or under pressure. They would not normally operate or be used fully in a vacuum. There is no indication of any vacuum pump there.

[fol. 728] In Fig. 3 there is no indication of either vacuum or pressure.

In Fig. 4 there is no indication of either vacuum or pressure.

In Fig. 5 there is no indication of either vacuum or pressure.

In Fig. 6 there is no indication of either vacuum or pressure, and the same is true for figures, 7, 8, 9 and 10.

In Fig. 11 we have the insertion of a valve, 179, which I believe was, by amendment, put in later, not in the original, which would indicate pressure, if valve 179 in this later amendment was put in, and pump, S, was used for more than simply feeding, it would give us a pressure in the system.

The Master: Suppose you wanted to use this alternative pressure of gravity, the flow of the oil being downward in the retort, E, where would the feed come in, referring to Fig. 2?

A. It would come in through the pipe labelled, 41, and the opening, 55.

The Master: Wait until I get that 55.

The Witness: This pipe (indicating).

The Master: Yes.

The Witness: And the opening which is about the point where the figure 55 is.

The Master: Yes.

The Witness: It would come in at that point, flow downward past the heater.

Q. The pump, S, on Fig. 11, is a pump, that is, "put into operation to draw the oil from the reservoir, B, and force the same under heavy pressure, through the preheater, D," just as Fig. 2 does, does it not?

A. It could be used in that same way.

Q. And it is so specified in the description, is it not?

A. That figure, you understand, was not in the original application, at least the valve 179, and I think the whole figure had been inserted by amendment later, and that would include the pump, S, in Fig. 11.

Q. Fig. 2 was the only figure in the original drawings, as filed, which showed the entire apparatus, as distinguished from the various separate parts shown in the other figures, excluding No. 11, for the moment, which you say was put in by a subsequent amendment?

A. That is my understanding.

[fol. 729] Q. And Fig. 11 also shows the entire apparatus, as distinguished from parts, does it not?

A. A simplified form of the entire apparatus.

Q. Now, when you first took up on your direct examination this patent, No. 1,320,354—I am now reading on page 406 of the printed record—you were asked what you understood to be the most characteristic feature of the apparatus described in this patent, and you replied:

"The use of an internal heater. This internal heater in this particular case is an electric heater which is independent of the walls of the retort."

You recall so testifying, do you?

A. I read it here, and I recall it also.

Q. And that heater, as you understand, is immersed in the oil and it does not contain oil?

A. No, the heater is made up of a series of hollow rods, or may be made of such a series—may be made up of a series of rods and we may have and do have the oil on the outside, and as well between the rods; that is, within the heater.

Q. When it is tubular, it is as described at line 100 of page 4 of the patent, No. 1,320,354 thus:

"It will be noticed that the electrode, 71, is tubular, thus enabling the oil to be converted to uprights through the same as shown by the arrow, 77, and in this way increase the converting capacity of the electrode, 71, by enabling oil to contact therewith internally as well as externally."

A. I find such a statement in the patent.

Q. Did you not say at page 410, when the Master asked you: "That the oil passes around the heater inside of the container upwards"?

A. I did.

Q. Now, taking one of those pipe-shaped electrodes of Fig. 10, and considering that as placed in a container-like, A, of Fig. 2, of this Adams patent, or of the application as originally filed, would not the oil passing through there upwards, under pressure, be a stream of oil passing through a heated retort of restricted cross section?

A. Yes, if we may think of this single unit of the heater as a retort, though primarily the heating is done by passing around the outside and between the rods of the heater itself. In other words, as I understand it, Adams considers the whole group of units as a heater rather than as a single, small tube.

Q. Did you not say over and over again in your direct [fol. 730] examination that Adams was the first to your knowledge to use an electric heater for this purpose of converting oil or cracked oil?

A. I do not recall that I said he was the first to my knowledge to use electric heating, but I think I can say that, or I can say that now, that he was, to my knowledge, the first to use electrical heating.

Q. Would there be any——

A. (continued). That is, internal electrical heating.

Q. Well, you are now shifting a little bit. Will you point out any reference in the prior art that shows an electric heater ahead of Adams for this purpose?

A. None was cited here, but it has been a common appliance used in laboratories, to externally heat our retort by means of electrical current, as by winding resistance wires about a retort, insulating them with, say, a she-t of asbestos paper. Yes, that is a common laboratory scheme that has been used for many, many years.

Q. Now, given Adams' scheme as he shows it in his drawing, would there be anything, in your judgment, other than the skill of the calling required of one to make the electric tube of sufficient size to take all the oil that is to be converted, instead of dividing it up into a number of separate tubes shown in Fig. 10?

A. It would be, essentially, a new invention.

Q. Why?

A. Because Adams does not disclose anything of that kind. He says that it is preferable,

"That the electrical heater, consisting of a circumferential series of such resistance rods, or pencils fastened together in pinion-like fashion to form a grid, and so connected that the current may flow through the same to best advantage."

This is his preferred procedure and his tube scheme, which you have mentioned, is not his preferred procedure.

Q. In your direct examination, when you were testifying about this patent of Adams, No. 1,320,354, you stated that the use of an electrical heater would be a much more convenient scheme, particularly when worked with small units, than an externally heated retort. And, again, you said the electric current scheme is more convenient, but much more expensive in operation.

Are those all the advantages that occur to you in connection with the use of the electrical heater? I mean to limit the question to a pressure system.

A. In the first place, I do not consider that the extra cost of the electrical current over other types of heating, is an advantage—though your question implies that it is an advantage.

Q. Well, now, let me just take your view, there.

A. May I know the page from which you read?

Q. Yes. I read from page 408 of the printed record, where the Master asked you, at the middle of that page, as follows:

"Just a moment. Is it your understanding there is something about the performance of the electrical heater which gives an advantage in the availing of pressure and vacuum?" And you replied, "Yes, it would be a much more convenient scheme."

A. Than an externally heated retort.

Q. And just below that, the Master asked:

"In other words, electric current would cost more than the conventional form of heating the chamber? The Witness: Yes, the electric current is more convenient, but much more expensive in operation." Those were your words [fol. 731] and not mine. Now, consider the question—I ask you whether there are any more advantages that occur to you than those you stated when the Master asked you about the advantages?

A. I did not state, on page 408, that. That was an answer as to the convenience, and not a question as being more expensive.

The Master: Now he asks you if you know of any other advantages which would flow from the use of that method.

The Witness: Yes, sir, minor advantages—such as the

more ready control of temperature of the heating element, and the possibility of using the same heating element for low temperatures or for any temperatures—that is dependent upon the amperage of electrical current put through—the possibility of even using the same apparatus for liquid phase cracking, and for vapor phase cracking. Those, I think, are the most obvious that occur to me.

The Master: Is there any advantage in the direct application of high temperatures to the point of incandescence, over other kinds of heating?

The Witness: Yes, there is. If incandescent temperatures are wished, it is easier to get them and to handle them by means of electrical heating, than it would be by means of external heating—using, say, such a material as iron, for the pipes.

Q. Now, is it not true that the internal electrical heater, such as Adams proposes in this patent, had a very marked advantage for applying heat to a high pressure operation, by avoiding transferring heat through the walls of the vessel which support the pressure?

A. Do I understand that this is a liquid phase or gas phase cracking, that is referred to in your question?

Q. Take it either way.

A. In the case of a liquid phase cracking it would have no particular advantage. In the case of a gas phase cracking, where we go to much higher temperatures, it might have an advantage, because when we reach those high temperatures, those higher temperatures, we begin to weaken the walls—if the walls are iron or steel.

The Master: Let me understand. Does the column of oil extend around and outside of the electrical heater?

The Witness: It would, in the liquid phase.

The Master: Exactly. If it be a vapor phase, the vapors would surround——

The Witness: The vapors would then surround.

[fol. 732] The Master: Now, the road of the radiation of heat through the walls of the vessel, would be that the heat would have to pass through the heated vapors or the heated liquid, before it could be transferred to the walls of the vessel—is that true?

The Witness: If we were using the electrical heat. If we were using external heat—which he asked for com-

parison with—where the wall is heated, and that heats the liquid, or the vapor——

The Master: It has to radiate inwardly, instead of outwardly.

The Witness: Yes, and in that case there might be an advantage with the electrical heating when it comes to the high temperatures of vapor phase cracking—temperatures of 1700 or 2000 degrees Fahrenheit.

Q. Now, in the liquid phase, the carbon tends to form a layer on the inner surface of the container, does it not?

A. It tends to form a layer of carbon, whether it is liquid or vapor phase—in either case. No, I beg your pardon—that would be only true where the surface, itself, is a hot surface. In the case where you had a surface in which you had the liquid hotter than the metal, it probably would not form.

Q. But I am now talking about a case where you have an externally applied heat against a metal container. In the conversion, carbon tends to deposit on the surface, right inside where the heat is applied, does it not?

A. Liquid phase or vapor phase?

Q. Let us take the liquid phase.

A. In the case of liquid phase, yes, if we are reaching active cracking temperatures.

Q. And that carbon layer is relatively a non-conductor of heat, is it not?

A. Yes, relatively, as compared with the steel.

Q. So that, if you desire a given degree of temperature in your oil, inside, you have to overcome, by the external application of heat, the metal container and the carbon deposit inside, in order to heat your oil to the degree desired.

A. That is true.

Q. And don't you know, that the effect frequently, is that the metal tends to break down—in actual operation—under those conditions, where there is a substantial deposit of carbon?

A. That is correct.

[fol. 733] Q. So that, in respect of that situation, the internal electric heater, has a decided advantage, does it not?

A. No. You get the same—not to amount to anything, because you get the same kind of carbon deposit on your heater, so that you would fill up between the rods of the

heater, and you would get on the surface of the heater, so that you would have, essentially, a single block of carbon, in a short time.

Q. But you don't mean to say that you mean that the electrical heater carries any pressure, under that condition?

A. No, but it would deposit carbon on it, which would mean that much more difficulty in handling it.

The Master: The heat transferrence would be retarded to that extent?

The Witness: Yes, and your conductivity on your rods would be changed, and you might get even some short circuiting of your current.

Q. Wouldn't your new deposit of carbon, itself, form an electrical resistance, that would develop heat?

A. I said a moment ago, in reply to the last question, that it would change the electrical resistance so that you would have to feed different amounts of current in. As the carbon deposits would increase—you would change the amount of the current being fed in.

Q. As indicated by your previous answers, a little while ago, that would be a matter of easier adjustment, due to the character of your heating element, would it not?

A. It would be not a particularly difficult one. You would have to feed in more electric current, to get the same amount of heat evolved.

Q. Now, where do you think there would be the short circuit that you injected into the situation?

A. A short circuit means a decrease of resistance between them, and if we would fill up the space in Fig. 6, between the rods, 10, of Fig. 6, you would have the whole mass as one block of carbon, and, thereby, a partial short circuit from the metal portions, 3, 6, 4 above, to the similar portions below, in Fig. 6. Wait a moment, 4 is not a metal. It speaks of it there—the rod, 90, is metal; the collar, 3, is metal, and the parts marked 6 are metal.

The Master: Where your current goes over 104, which is one of your poles to——

The Witness: I was using Fig. 6 as a heating element.

The Master: —to the bottom, in connection with 155, [fol. 734] where is there any opportunity for an arc getting in anywhere along the line?

The Witness: Not an arc, but a reduction of resistance. An arc would be a complete removal of resistance, and this is a partial reduction of resistance, if we filled up the spaces in Fig. 6, by carbon—we would have a solid block of carbon, reaching over to the metal division at each end, and the resistance would be, perhaps, one-third or one-half what it was originally.

Q. Well, are you enough of an electrician, Professor, to know that this carbon deposit is of much higher electrical resistance than the rod?

A. I would presume that it would be within the graphite—and that is why I spoke of it as being much more, because its total arc in there would be—judging from Fig. 7—the cross section—would be five times as much as the rods—so that, if it had five times the resistance, it would still be equal to what I said a moment ago—possibly doubling the conductivity—that is, having the resistance of the whole, as a unit.

Q. A short circuit is not what you have been talking about. Isn't a short circuit a drop of resistance to substantially zero?

A. That is one kind of a short circuit—a complete short circuit. This is a partial short circuit, only, and a reduction of resistance.

Q. Just for the purpose of the present discussion—that is what you call a short circuit?

A. No. It is always true. It is a circuit through a less resisting material, or a circuit which is through a less resisting circuit, is a shortened circuit, and——

The Master: You don't mean a short circuit, at all, in the technical sense. You mean a lowered resistance?

The Witness: Yes, a lowered resistance. It is often spoken of as a shorted circuit.

Q. The fact still remains, however, does it not, that the walls of the retainer, in which this internal electric resistance is placed, would not be softened by this internal heat, isn't that true?

A. Not if they were the ordinary type of steel, or similar strong material.

Q. Such as these things have been made from, from time out of mind?

A. I don't know what you speak of.

[fol. 735] Q. Oil handling apparatus is what I thought we had been talking about all the time. Didn't you understand it so?

The Master: What he means, Doctor, is this: Assuming that all you say about the lowering of the resistance by the deposit of carbon on the electrodes, there still remains in the apparatus, this advantage, that by the internal heating, as compared with external heating, you avoid any burning spots, on your tank, A?

The Witness: That would be correct, if we are using such materials as iron and steel for the container.

Q. Is the use of an internal electrical heater feasible, in your opinion, where the electrical power is available at low cost?

A. No, because we don't have, ever, electrical heat at low enough price to compete, really, with other types of——

The Master: Heating by waste gases?

The Witness: Well, not only waste gases, but gases that you have to pay for.

Q. Now, answer the question.

Mr. Rauber: I think he has answered the question.

Q. Do you understand that you have answered the question?

A. I thought I had, but I can go into it in more detail.

Q. Let me ask you this, if you think you have answered that question. Do you say that it is not practical, in your judgment, to use an internal electrical heater?

A. Do you mean commercially, or scientifically?

Q. Commercially, I mean.

The Master: For economical reasons.

A. It is not practical, because, for example——

Q. Well, that is an answer. Never mind an explanation at the present time. I am just trying to get answers——

A. I would like to finish my answer.

Q. I think that is an answer to the question. I asked your opinion on that one point.

The Master: I think you have answered it, Doctor.

Q. Now, this morning, when we were talking about the application for the Adams patent No. 1,327,263, the appli-

cation being known as Serial No. 618,011—you said that the applicant's abandoned application, No. 535,879, had not been brought to your attention, and that, at that time, as you knew, was not referred to in the application, in the file wrapper, No. 618011.

I will call your attention to Paper No. 36, being the deci- [fol. 736] sion of the Examiners-in-Chief, on appeal, dated August 24, 1915, and to what they say in that opinion, as follows:

"And it is a fact that in applicant's abandoned application, No. 535,879, which is practically the same as the present application, applicant's attention was called to the fact that the vacuum process was alternative to the pressure process, and it was held that he should point out when one is used and when the other is used. Applicant complied with this objection by limiting the present case to the pressure method, thus differentiating it from his patent No. 976,975"—

That fact had escaped your attention, had it?

A. Yes. Understand, however, that 976—well, the direct statement in his first few lines of his application, that it is for the method used in No. 976,975, seems to be contrary to this statement in this Paper No. 38, page 3.

Q. Well, now, wait. Don't try to argue your entire case at once. We are now talking about the process patent, No. 1,327,263, and I am calling your attention to its file wrapper, and to the fact—which you say you do not know, or did not know previously—that that file wrapper contains reference to the applicant's abandoned application, No. 535,879, which I stated, this morning, was filed in December, 1909.

A. I confused the two file wrappers, and the two applications which we have been discussing.

Q. And the point I am bringing out, at the present time, is that you did not know, until now, that that abandoned application, of December, 1909, to wit, No. 535,879, was referred to in the file wrapper, No. 618,011, out of which eventuated the process patent, No. 1,327,263.

A. That is correct—I did not recall it. I may have known it earlier, but I had forgotten it.

Q. Now, when this patent, No. 1,327,263, of Adams, was first brought to your attention on this record, by counsel

for the Government, you were asked if you had read and understood its disclosures, and you said, "I have, and I understand the essentials." The next question was, "From your reading of the specification of Adams patent, No. 1,327,263, what do you consider to be the most characteristic of the process described in this patent?" To which you replied, "The intense incandescent temperature used in the cracking, temperatures used in the range of 1750, or even 2000 degrees Fahrenheit." Do you recall that testimony?

A. Yes, I recall that testimony.

Q. Did you understand that it was the oil, or the pipe through which the oil flowed, that was to be at an incandescent temperature?

A. Not the pipe. It is hard to think of the oil actually [fol. 737] being incandescent, but it is at a temperature at which a solid body would be incandescent. Vapors are not ordinarily, even though very hot, incandescent.

Q. Now, where, in this Adams patent——

A. I beg your pardon. I am confusing again, here, the one that we were discussing a few moments ago. In this particular case—this is not the electrical heating—this is the one where we have a pipe-like retort, externally heated, and the pipe is at the incandescent temperature, and, correspondingly, the oil at approximately the same temperature.

Q. Why do you say approximately the same temperature is flowing through the pipe?

A. Because there may be a coating of carbon on the pipe, which would lower the temperature somewhat from the inside, as compared with the pipe material.

Q. How much of a coating would there be, in view of this helical scraper, 69, in this Adams patent?

The Master: That is not a scraper, at all?

Mr. Cooper: Well, I will come to that in a moment, your Honor.

A. There would be a layer of carbon in an amount equal to the difference of the diameter of the wall, 12, and the helical collar, or worm, 69, which does not quite engage the surface, of the chamber, 12. From the drawing and from the context, I would expect that to be relatively thin—perhaps——

The Master: Equal to the clearance between the helical collar and the wall?

The Witness: Roughly so, yes, sir.

Q. That is, there has to be a mechanical clearance, in order for the helix to rotate?

A. Yes, any excess would be scraped off by the helix.

Q. And it would be simply the judgment of the designer, to leave enough clearance so that the helix would turn?

A. Yes, if it was in direct contact with it, due to the temperature there would be a cracking of such a chamber, of graphitic carbon, if that was the wall material.

Q. And you understand that in connection with what the specification says, on page 4, line 71, in these words:

"Another specific application of the helical collar, 69, when in rotation, is to remove and throw down the accumulation of scales and particles of carbon and coke which might adhere to the interior surfaces of the cracking chamber, 12"?

A. Yes, that is a scraper effect.

[fol. 738] Q. Now, as a designer of apparatus of this sort, can you say how much that clearance would be?

A. Probably an eighth of an inch. It might be more, or it might be less, but that would be a fair clearance. It would be probably not less than that. It would depend, of course, on the size of the apparatus. If the pipe, 12, was only four inches in diameter, it might be worked at even less, but if it was as much as six inches in diameter, it probably would not be less than an eighth of an inch.

Q. And assuming that there is as much as an eighth of an inch, what practical effect would that have on the difference in heat, between that applied to the outside of the chamber, and the temperature of the oil?

A. It would have to be—well, it might readily amount to ten per cent reduction. However, it is to be noted that the applicant—the patentee, here—seems to think that the two are at the same temperature—that is, the oil and the well. At least, he speaks of it not being economically or commercially practical to bring about a cracking under heat of 2000 degrees Fahrenheit.

Q. Where does he say that in the specification for this patent?

A. He doesn't say it in the specification, in the patent, but does say so in a letter to the Examiner, on May 2nd, 1912.

The Master: Do you understand that this rod, 23, and the scraper, 69, that helical collar, rotated in a clock-wise movement, would lift the column of oil in the chamber, 12?

The Witness: No. It would be the reverse. It would be counter-clockwise, to lift the—that is, looking from above, it would be counter clock-wise, if it was wanted to lift the oil—not clockwise.

The Master: It looked that way to me, exactly as it is geared there, at 35,——

The Witness: The rod, 23——

The Master: Revolves and rotates clockwise. The pressure on the column of oil, by that helix, is upward, isn't it? It is stated in the patent that the first function of that collar is to agitate the oil, 5, in the chamber, 12.

The Witness: If the oil was running in from pipe, 11, through valve, 15, pipe, 16, into the lower portion there, and passing upward through the retort, it would agitate it more if it was running contrary to the movement of the oil.

The Master: Whether it would agitate it more or not—the question is whether it doesn't lift it, and at the same [fol. 739] time agitates it, and if that wouldn't hinder the deposit of carbon on the walls of chamber, 12?

The Witness: If it was rotating clockwise, you would have a scraping off of the carbon, and the carbon would drop down. In other words, it would be below the helix, and it would cut off and drop down. If it was rotated counter-clockwise, the carbon that was cut off would tend to ride on the helix.

The Master: Well, maybe I don't read it right.

Q. Now, you have not yet told me how much difference there is between the temperature of the oil, and the temperature of the pipe, 12—the outside temperature of that pipe—in carrying out this Adams process, in the apparatus show in patent, No. 1,327,263?

A. I have told you—I have said that it was about ten per cent of the temperature—it would be approximately ten per cent of the temperature.

Q. Do you remember the Kittle patent—the French patent, No. 417,532, where he says the temperature of the bath should be at least 700 degrees Centigrade, but by reason of the absorption of heat by the oil, the temperature of this will ordinarily be about 500 degrees Centigrade?

A. That is correct. It must be borne in mind that the bath, here, would correspond to the hot gases, not to the wall. The tube of the Kittle patent is undoubtedly cooler, due to the oil flowing through it, than the bath, itself. In fact, the oil is said to be at 500.

Q. Now, in Kittle, the bath that I referred to in my previous question, was the melted lead bath, in which the pipe, D, is immersed—isn't that correct?

A. That is correct.

Q. And you have practically identical conditions, haven't you, in that Adams patent, in that whatever is inside of the pipe, D, in Kittle, is in contact with the walls, whose outside temperature, which must be, as he says, of the order of 700 degrees Centigrade?

A. He doesn't say that the walls shall be that. He says that the bath shall be that. Now, the pipe, D, having a cold liquid passing through it—or a cooler liquid passing through it—would be at a lower temperature, than the bath, which is heating it. There will be, always, of course, a differential between the bath and the wall of the tube, and there will be a second differential between the tube and the oil. This bath would correspond to the vapors in the space between the tube, 12, of the Adams patent, and which [fol. 740] do the heating of tube, 12. We have not discussed as to what these hot gases—burning flames—what the temperature of those is.

Q. Do you mean to tell me, as a chemical engineer, that in an arrangement such as Kittle, where pipe D, is immersed in melted lead, that the exterior of the pipe, D, is at an appreciably lower temperature than the 700 degree Centigrade which he gives as the temperature of melted lead?

A. Yes, indeed. And, in addition, I may say that the temperature that he is giving for the melted lead, is the approximate temperature of the bath, as a whole, and not that of the lead, which is in closer contact with the pipe, D—it would be the coolest part of that bath, and the bath would not be uniform throughout its whole mass.

Q. Are you sure of that as of every other fact that you have testified to in this case?

A. I am quite sure of it.

Q. And in your judgment, is the differential between the exterior of the pipe, D, in Kittle, and the lead bath?

A. And by the lead bath I presume you mean the average temperature of the lead bath, rather than the particular spot in touch with pipe, D, or the particular spot which is the hottest—say that just over the flames—which is being heated? Which part of the lead bath do you refer to?

Q. Did you ever test a lead bath in which there was any sensible difference in different spots in the bath?

A. I have tested—I don't recall that I ever tested a lead bath, but I have tested other baths, and it is quite common to have as much as fifty, and even—for high temperature work—even a hundred degrees difference.

Q. What kind of a bath was that?

A. The best known ones are a question of air baths, heating ovens, and in the heating of the ovens, which are supposed to be at nearly constant temperatures, we find large differences, even though they are highly insulated.

Now, in the case of liquid baths we find differences which would correspond, here, to this—though, as I say, I have never, as far as I recall, tested a lead bath. I have, though, tested organic liquid baths and water.

Q. How does the heat conductivity of air compare with that of melted lead?

A. The heat conductivity of any vapor is less than that of any metal, so far as I recall, but I was referring, not to the question of conductivity, but to air baths, which are supposedly the same throughout their entire space, and were not losing heat, to a moving body, such as there is in this case.

[fol. 741] Q. Is that the only answer that you can give, as a scientist, who is held up here by the Government of the United States as the greatest authority living, in this art, and who have, yourself, stated that there is probably, in this country, and probably nowhere in the world, anyone who knows as much about shale oils as you do?

A. You have misquoted me. This is the third time, I believe, that you have misquoted me in today's testimony.

Mr. Cooper: I move to strike that answer out, and I repeat the question.

The Master: I don't think that is a fair question, Mr. Cooper.

Mr. Cooper: Well, I don't think it is a fair answer, for him to tell me that I have misquoted him. He has no privilege of that kind.

The Master: I think the answer is as fair as the question.

Q. I will ask you once again: How does the conductivity of air compare with the conductivity of melted lead?

A. As I said, it is very much less. Any gas has a lower heat conductively than the metals.

Q. And it is less than 100ths as much, is it not?

A. I do not recall the figures, at all.

Q. Will you dispute my statement that it is not more than 100ths?

A. Judging from your manner of misquoting, I would have no faith in the figure that you give.

Mr. Cooper: I move to strike that answer out.

The Master: It may be stricken out. There is no necessity of getting any ill feeling between counsel and the witness in the matter, which probably will be regretted.

Mr. Cooper: Will you read the question?

(Question read by the stenographer.)

The Master: If you can give any near ratio between water and lead conductivity, why, you may give it.

Mr. Cooper: No, air and melted lead.

The Master: Or air and melted lead; if you can not, just say so.

A. I can not give any numerical figure for it.

[fol. 742] Q. In this arrangement of Adams, Patent No. 1,327,263, what would be the temperature of the walls of the member, 12, when the oil temperature is 900 degrees Fahrenheit?

A. It is difficult to say but it would be approximately a thousand degrees. It would depend upon the thickness of the carbon; it would depend upon the speed with which the oil was passing through the retort; possibly, upon other factors, but roughly a thousand degrees, possibly as much as eleven hundred.

Q. Would your answer be the same if I had specified the outside temperature of the tube of member 12?

A. I would say that it would be perhaps a little higher, but approximately the same.

Q. It would be something like a thousand degrees Fahrenheit with an oil temperature of 900 degrees?

A. Yes, as I said, depending somewhat upon the thickness of the carbon on the inside of member 12, depending also upon the speed with which the oil is passing through the chamber 12; depending somewhat upon the thickness of the wall, but in that general range.

Q. Well, assume a half inch wall, and what you would estimate it to be, an eight inch thickness of carbon, then answer the question under that assumption.

A. Again, we would have to assume the question of speed of the oil through there and so on, but it would be, as I say, roughly a thousand degrees.

Q. And if that pipe 12 were made of steel, that would be a red—that is, a visible red heat?

A. You probably would not recognize it as red heat in the daytime. You might if you had it at night and no other light about the place at that temperature, but I would say that in the presence of the burning flame about there it would not look red; it would not look to be as high as an incandescent temperature.

Q. Now, in your direct examination you said, on page 415, that a visible or bright cherry red, to a full white heat would be approximately from 1150 Fahrenheit to 1850, which latter you said would be as low as you would count a full white heat; is that misstating your testimony?

A. No. I think that is still there. That is, you can begin to see, as the temperature is here, as I said, a thousand degrees Fahrenheit—you can see that at night in the blackness; it has a color to it, as low red heat.

Q. But that question——

[fol. 743] A. (continued). Then if you will go higher than that, or the temperatures I suggested there, 1150, which is 150 higher, you would begin to see it without any particular trouble, even in the daytime.

Q. But that question of the Government's counsel to you:

"About what temperature of Fahrenheit would a temperature of visible or bright cherry red, to a full white heat, be, approximately?"

And you said:

"From about 1150 Fahrenheit to 1850."

Did you mean 1150 Fahrenheit to represent your then idea of a visible or bright cherry red?

A. Yes. In the daytime you can see that and you would call it a low red heat, or a cherry red heat; whereas, if you take it up to 1850 you would have it at a temperature which would be approximately a white heat; anything above that is counted as a white heat.

Q. Refer again to this Kittle French Patent No. 417,532, and assume that instead of applying the heater, F, to melt the lead bath, it were applied directly to the coils or coil pipe, D; what in that case would be the differential in heat between the exterior of the pipe and the oil?

A. I think you are in error in speaking of F as a heater; F is a tank.

Q. Excuse me; H is the heater. I got the wrong letter of reference; H.

A. I understand that the bottom of the tank, E, would then be removed and that the hot gases from H would pass up in direct contact with the pipe, D.

Q. Or the heater applied to the coil in any well known manner?

A. I would expect it to be, in this particular case, about 10 per cent. That is, about 100 degrees Fahrenheit, provided there was no large deposit of carbon. That is only an estimate, of course.

Q. How would you remove the deposit of carbon if it were sensible on the interior of the coil part, D, in Kittle?

A. It is removed by the flow of vapors through there; the speed of the vapors passing through the pipe, D, will have the effect of carrying away carbon. Of course, his use of the lead bath is in part to avoid local overheating with formation of carbon.

Q. Would not carbon deposit at 700 degrees Centigrade in the bath?

[fol. 744] A. No, carbon does not deposit in the bath.

Q. I say, would not carbon deposit in the interior of Kittle's pipe, D, when immersed, as he there shows, in a lead bath of the temperature of 700 degrees Centigrade?

A. There may be some deposition of carbon there, but

it is essentially washed out and into the tank, F, by the speed and flow of oil, patricularly oil in the form of vapors; partly as vapors, partly as liquid.

Q. Why would there not be the same washing out in Adams on the interior of his pipe 12?

A. In the first place, he uses higher temperatures, which would mean greater adherence of carbon; and in the second place, the speed of the oil past the surfaces is much less, and as such, the friction of the oil upon the surfaces to keep the surfaces clean would be much less.

Q. Why are you so positive that the speed of Adams' oil would be so much less than Kittle's?

A. My general impression, from reading Adams, is that the pipe 12 is not less than, say, four inches in diameter. It may be somewhat more. With that diameter the amount of space occupied by the oil is very much larger than with Kittle. The heat supply is, in one case, six burners, and in Kittle it is 9 burners. In Adams it is six burners and in Kittle it is nine burners.

Now, if we have the same amount of heat supply, and roughly the same degree of cracking taking place, and the space not occupied by that center portion of the pipe 12, would be many, many times the cross section of the interior of the pipe, D, of Kittle; and consequently, the velocity would be correspondingly less. If it was ten times the cross section, it would have perhaps 1/10th speed.

The Master: If the flow of oil revolves, as you claim, in a direction in which it actuates the liquid downward, the flow of the liquid through the column 12 would thereby be retarded, would it not?

The Witness: Yes, it would largely be not a direct, vertical flow, but a circumferential flow.

Q. Do you mean to rely on the fact that in Kittle there is 50 per cent more of heat supply than there is in Adams for your conclusion that, therefore, Adams has a higher heat supply?

A. No, I did not say that.

Q. I do not understand how the number of burners in [fol. 745] Kittle has anything to do with it when the burners are not applied to the coil itself, but to a molten bath.

A. It is to be expected, though it is not shown, that the bath, E, would have insulation about it, and consequently,

the large part of the heat applied by the burners, H, would be taken up by the oil in vaporizing the oil and in cracking the oil, and similarly in Adams, the burners, 12, would supply heat, which very little of it would escape, through the vessel made of fire brick, 67, and that would serve as insulation there. So that in each case we ought to have the heat added by means of the burners largely carried away by the oil.

Q. Now, give all the bases that you find in the specifications of this Adams statement for your impression that the pipe 12 is four inches in diameter.

A. I do not recall that a dimension is given. It, however, is stated——

The Master: You mean in Adams?

Mr. Cooper: In Adams.

The Master: The pipe, 12, is only four inches in diameter?

Mr. Cooper: The Professor said in his last statement that it was his impression that the pipe, 12, in Adams, was four inches in diameter. I am asking him for the basis in the specification——.

The Witness: No, I said it was at least four inches in diameter.

Mr. Cooper: Have it as you will.

A. Page 4, line 9:

"The oil-containing cracking chamber, 12, may be constructed of various heat enduring metals, alloys, carbons, graphite and graphitic carbons and other materials known to withstand a constant and varying temperature of high intensity."

If we are going to construct of carbons or graphite, it would be doubtful to construct it much larger than that. It might, perhaps, be easily twice four inches.

The Master: Does that not become entirely academic in view of the plain instruction on page 7 of the patent, that by the terms "incandescent and low red and a full white heat" it is only meant that the temperature of the oil in the chamber be raised to a temperature at which desired cracking may take place?

Mr. Cooper: Of course.

The Witness: That, your Honor, might be true if it was not that the applicant actually speaks through his attorney, in paper No. 3, page 2, lines 2 to 9:

[fol. 746] "'In his spiral or serpentine tube, supported over a fire—'"

Adams is speaking now, through his attorney, or possibly by himself. (Referring to document.) Signed by himself. Speaking of another process:

"'In his spiral or serpentine tube, supported over a fire, wherein he maintains an average of 700 degrees Fahrenheit to a thousand degrees Fahrenheit' he had no idea of cracking kerosene or transforming other light Baume gravity oils into benzines and gasoline, for it requires a heat beginning at 1750 degrees Fahrenheit to bring about any appreciable cracking of the heavy petroleum oil molecule, and it is not economical or commercially practicable to bring about such transformations under a heat of 2,000 degrees Fahrenheit."

So that the applicant really considered that 1750 to 2,000 degrees Fahrenheit was required for a temperature of the oil.

The Master: I think that is a misunderstanding. The plain statement of the patent is that the heat will vary in accordance with the material of which it is composed, and the temperature necessary to secure desired cracking of the oils will vary with the character of the material under treatment. Assuming that you put crude oil in, perhaps you would have to run the heat up very high, but suppose you were putting other lighter oils through it, you would not have to run it at anywhere near such temperature.

The Witness: The applicant was having to distinguish between Benton and his process at the Patent Office and Benton uses a temperature of 700 degrees Fahrenheit to a thousand degrees Fahrenheit.

The Master: What did you say the temperature necessary to crack ordinary gas oil was?

The Witness: Did I or he say?

The Master: Did you.

The Witness: Around 750 Fahrenheit.

Q. Now, you spoke, if I understood you correctly, of the pipe, 12, of Adams as containing a considerable body of oil, if I got you correctly. I want to call your attention to page 4, lines 36 to 47 of the specification, wherein he says:

"To this end in the embodiment of the apparatus shown by Fig. 2 I have provided a cylindrical core or sheath, 68, of a size designed to fill a large part of a central space within the cracking chamber, 12, and to displace a considerable amount of oil which would otherwise be contained therein, thereby compelling said oil as may be located be- [fol. 747] tween this core, 68, and the containing chamber, 12, to come into more or less direct contact with the intensely heated walls of the cracking chamber, 12:"

Did you have that in mind in making the answer?

A. I did, but I was comparing it with Kittle which has a pipe, D, with an interior diameter of less; of approximately or a little less than the size of my lead pencil.

Q. Now, how long would such a pipe as that run in an arrangement such as Kittle shows without clogging up with carbon?

A. I do not know.

Q. If it did clog up with carbon how would you get the carbon out of the Kittle, with a helical pipe?

A. Kittle apparently considers that the process will run an indefinite time, that is, run continuously, for he says:

"that the oil and impurities are led through the pipe, f-2, of the cooling worm, J, into the reservoir, K, with the overflow, K-1."

Further, he calls attention that by the present invention, "local heating of the oil being completely avoided—"

In other words, he seems to feel that the amount of carbon will either be zero or more likely very low in amount.

The Master: That does not answer the question. The question was, if it did clog up with carbon, how would you get it out?

The Witness: You could get it out by still keeping it hot, stopping the oil and blowing air through. Of course, that would presume that it was not completely clogged, but only partially clogged.

Q. Did you ever try that, Professor?

A. Not on a Kittle apparatus. I have never operated a Kittle apparatus.

The Master: How about taking it apart and letting the coil cool, and running a piece of wire through it or something of that kind?

The Witness: I have done that on an oil coil.

The Master: It would be perfectly obvious if it got sufficiently clogged, you would have to stop your operation and clean up, that is all?

The Witness: Yes.

Q. What have you to say as to the practicability of any commercial oil cracking process that applied a bent tube, including a helical one, or serpentine one, as distinguished from a straight tube?

[fol. 748] A. In general, they are less desirable, because if carbon forms in the tube, it is more difficult to clean; however, it is, I believe, possible to use a helical tube in a vapor phase process, provided the vapor flow through there is at high enough speed——

Q. What speed?

A. (continuing). Particularly in the presence of steam.

Q. What speed?

A. Greenstreet process, I think, has operated—I am told that it has operated at the speed of nearly a thousand feet a second, and the statement is made that the tubes keep clean in such a case as that. This is, as I say, only secondary evidence, though I have not operated it, and do not have the details.

Q. Do you believe the statement you have made as secondary evidence?

A. Yes.

Q. Now, making the assumption that it is a liquid phase process, what have you to say as to the practicability of using a bent tube such as is covered by my previous question?

A. I would say, in general, it is less desirable than a straight tube scheme, due to the difficulty of cleaning the tube, if carbon should attach itself to the surface of the tube.

Q. Now, Professor, I do not want to be charged unnecessarily with misstating what you have said, and I want to put the plain question to you:

You mean by "less desirable" in your last answer the same as "wholly impracticable"?

Mr. Rauber: May I ask you to read the last answer?

(Answer read by the Reporter.)

The Master: Do you mean the coils were wholly impracticable?

A. I would say the coils might be made practicable, but as the process is ordinarily carried out, they would give lots of trouble. A bent tube, on the other hand, would be possible to operate; that is, not a complete coil but a bent tube could be operated much more esaily, and probably it could be cleaned fairly well.

Q. Describe exactly what you mean by "bent tube" in the exception you have now?

A. Well, a tube which is a hairpin tube, so that the tube where it was joined, say, to other tubes or constituted a single tube, were all at the same end of the heating chamber. Such a tube would be less desirable than a straight [fol. 749] tube for the reason indicated, but it might easily be practicable to use it in a commercial process.

Q. Then, would you say that a coil——

The Master: You mean such tubes as are shown in Cross, No. 1,423,500?

The Witness: They are undesirable. I would say it would be impossible to clean such a tube as that.

The Master: Even if you detach its outer sections beyond the wall of the furnace, and clean them practically as horizontal tubes?

The Witness: The previous patent of Ellis in my book, and I think it is the same in yours, gives an end view of the tubes on that.

The Master: Yes, I see it.

The Witness (continued): What accomplished the same thing, but the end of each straight tube can be opened and gotten at, and that, in general, has a more desirable form.

The Master: He referred to a hairpin tube, and that recalled to my mind this disclosure, of things that look like hairpins.

Q. Then, any such arrangement as Cross, No. 1,423,500 ordinary engineering would provide headers where the

bends of the tubes project outside of the furnace, would it not?

A. I do not think that Cross had this in mind, or he would have so indicated, just as Ellis.

The Master: Would not any engineer or steamfitter know how to adjust that?

The Witness: I would think a capable man would. He speaks of it, though, in line 62, as "a continuous coil of pipe." He is using the term "coil."

The Master: That is not at all in conflict with the idea of bends on the tubes on the outside of the wall, which might be detached for cleaning.

The Witness: Ellis, in the patent that we refer to a moment ago, speaks of it as "a battery of heating tubes," making a distinction between a coil and a battery.

Q. Where, in any single item of the art that you have considered in this case, of a date prior to January 1st, 1910, do you find described a process of converting oil, which consists in continuously passing oil in a relatively thin stream, under pressure, through a pipe-like heater to a larger container or vapor dome, in open communication therewith, heating said oil locally as it passes through said pipe-like heater to a cracking temperature, maintaining [fol. 750] substantially the same vapor pressure on the oil in the heater and vapor dome, and condensing the vapors in the dome?

The Master: You have got to find that all in one disclosure.

The Witness: I think I have got five that will give it.

The Master: I mean the entire combination has got to be all in one disclosure, not made up of parts taken from five separate disclosures.

A. The first citation I would give would be Dewar and Redwood, in which it is heated in the pipe-like heater or retort, C, and carried through the process. This is "Petitioner's Exhibit No. 103."

We have the same described in Redwood's Petroleum, 1896 Edition, Volume 1, pages 321, and 322.

In Schuchow and Gavrilow we have exactly this process described:

"We are continually passing oil in a relatively thin stream under pressure through the pipe-like heaters, D, and D','' which may be either coils, as he illustrates, or as he states, straight pipes.

"into the dome, A, which is in open communication therewith, heating the oil as it passes through the pipes, and maintains the same vapor pressure on the oil in the heater and in the dome, and then conducts off through the valve M, and the pipe, E, to the condenser.''

In the German patent to Clercq, patent Gesellshaft, we have a process for converting the oil in which it passes through the coil, A, in the furnace, by means of the pump, C, into the container, D, all under the same pressure; and then the vapors are conducted off through the pipe, E, and then condensed in the condenser shown in the figure.

The Master: All under uniform pressure from heater to condenser?

The Witness: There is no requirement, your Honor, in this question that the condensation shall be under pressure, but it is only that the pipe-like heater and the dome shall be under the same pressure. It is really Claim 8 of the Adams that he is reading.

A. (continuing): In Kittle, we have the passing of the oil from the container, A, through the valve, A1, from the pipe, D, into the larger container, F; heating the oil locally as it passes through the coils, D, to a cracking temperature, having substantially the same vapor pressure on the [fol. 751] oil in the pipes, D, and in the vessel, F, and conducting off through the pipe, *f*, to the condenser, G, and condensing the vapors.

Q. I call your attention to the fact that the Kittle patent is beyond the date that my question specifies.

A. I beg your pardon, sir. I did no recall that there was a date limitation. What is that limitation

Q. January 1st, 1910.

A. That, then, would not be within it. The Kittle reference should have been omitted on account of the date limitation.

As another reference, Pielsticker's British patent, No. 1,308, of 1891, being "Petitioner's Exhibit No. 133.'' The oil is forced in by pump, C, through the coils, D, and into the large container, I, in open communication therewith.

The oil is heated as it passes through the pipes, D, to a cracking temperature, and the oil has substantially the same vapor pressure in the pipes, D, and in the large container, I, and conducts off the vapors through the valve, R, and pipe, Q, to the condensors, U, and U'.

In "Petitioner's Exhibit No. 134," Pielsticker's United States patent, No. 477,153 of 1892, we have the oil pumped in by means of pump, C, through pipe, K, into the coils, D, where it is heated to a cracking temperature, and then passes under essentially the same vapor pressure, and then into the larger container, L, out through the valve, B, and pipe, S, to the condensers, U, and U'.

These, I believe, are all disclosures of the process which you described.

Q. Now, in your last answer, after you mentioned the Dewar and Redwood patent, and the Redwood publication of 1896, and when you took up the Schuchow and Gavrilow Russian patent, you said of the latter, that it described exactly this process. Did you, by that language, intend to make a differentiation between what that Russian patent, with reference to the subject of my inquiry, and what the Dewar and Redwood and the Redwood patent and publication show?

A. No, only that it is a little plainer in the description.

Q. Other than being a little plainer in the description they are all the same, in your judgment, in their relation to the subject of my inquiry?

A. I think they are all complete disclosures of that process.

Q. Now, from your examination of the file wrapper known as 618,011, you discovered, did you not, that the Patent Office cited the Dewar and Redwood and the Red- [fol. 752] wood publication, and that the patent of Adams, No. 1,327,263 was allowed by the Patent Office Examiners, notwithstanding the references?

A. Yes, on the basis that the Dewar and Redwood specification did not disclose a continuous process.

Q. And claim 8, which has been the subject of my recent examination of you, was itself before the Board of Examiners-in-Chief at that very time, was it not, as shown by paper, No. 35½, which contains it?

Mr. Rauber: Your Honor, I would like to object to that question. This is all on the record, and I do not think it

makes any difference whether Dr. McKee testifies that Claim 8 was allowed or not, it would be the same.

The Master: He is cross examining him.

A. That is correct, claim 8 was claim 8 at that time and was one of the claims under discussion.

Q. And in your immediately preceding answer, when you made reference to the ground on which the Board made the allowance, you referred to this language, did you not:

"We are of the opinion that it is too uncertain whether the Dewar process was or was not continuous to reject applicant's claims thereon." That is what you referred to, was it not?

A. No, I was referring to page 3, which is essentially the same.

"Applicant contends that this process was not a continuous one, but was intermittent. There is nothing in the Dewar et al. specifications to indicate whether that process was an intermittent or continuous one."

My own view is that there is something in there.

Q. Now, I did not ask you for your view.

A. (continued): But they say there was not, and it was to that that I was referring.

Q. Are you opposing your opinion to that of the duly constituted tribunal of the patent office?

A. Yes.

Mr. Cooper: There are two statements I would like to make before proceeding with the cross examination; one is with reference to an inquiry that his Honor put to me yesterday as to why there was not a reference in the specification of patent, No. 1,327,263, to the application that was filed in December, 1909.

I find there is such a reference on page 3, in the paragraph running from line 41 to line 50, inclusive.

Q. In your direct examination, at page 418 of the printed record, the following appears. The question was: "In the specification of the Adams patent, in the file wrapper of patent, No. 1,327,263, do you find any description of a valve—pressure release valve at the outlet of the condenser or anything that would indicate that that feature was characteristic of Adams and Benton?

Your answer was "I do not."

The Master: That "Benton" should be "Burton," should it not?

Mr. Rauber: "Invention" I think it is.

Mr. Cooper: All right, take the question with that understanding, that it reads—

The Master: "Adams' invention."

Mr. Cooper: That that feature would indicate it was characteristic of Adams' invention, your answer was "I do not."

Q. I call your attention in this Adams patent, and on Fig. 1, to a valve at the upper end of the pipe, 50, which is at the extreme left hand of the figure, and the valve in the pipe, 40, which is near the bottom of the structure marked "collector." In that apparatus that is shown in Fig. 1, as you understand it from reading the specifications, would [fol. 753] those two valves be pressure release valves located at the outlet of the condenser?

A. These might be used as pressure release valves.

Q. Where, in any single item of the art that you have considered in this case, of a date prior to January 1, 1910, do you find described a process of treating high boiling hydrocarbon oils to convert a relatively large percentage of the same into lower boiling hydrocarbon oils, which consists in continuously supplying the high boiling hydrocarbon oils to a closed chamber, and subjecting the high boiling hydrocarbon oils in liquid form to a cracking temperature in said closed chamber under pressure to evolve vapors of the desired lower boiling hydrocarbon oils and condensing said vapors by passing them through connections to a condenser; all of the connections to and through the condenser being in open communication so as to insure a substantially uniform pressure from the heater to the condenser.

The Master: That is Claim 1 of the patent. In answering that question it is only necessary for you to name the apparatus, without going fully into any further description of it. It has been described many, many times in the record from these prior references. All you have to do is give the name of the reference.

"Petitioner's Exhibit 106," Laing, particularly pages 158 and 159.

"Petitioner's Exhibit No. 103," Dewar and Redwood.

"Petitioner's Exhibit 107," Redwood, and in the light of "Petitioner's Exhibit 106" we can use "Petitioner's Exhibit 129," Schuchow and Gavrilow.

Q. That last is not a single reference.

A. (continued): No. 133——

Mr. Cooper: I asked for a single reference, your Honor. With reference to one of these he says "In view of something, then Schuchow and Gavrilow applies." I asked him to confine his answer to a single reference.

The Master: The answer may be good, Mr. Cooper.

A. (continued): And in view of the same reference, "Petitioner's Exhibit 103," Laing, "Petitioner's Exhibit No. 133," Pielsticker British Patent, and similarly "Petitioner's Exhibit 134," Pielsticker's United States Patent——

Q. Now, what difference do you make with respect to the complete fulfillment of the process between Dewar and Redwood and any of the other four citations you have just [fol. 754] made, to wit, the Laing article, the Redwood article, Schuchow and Gavrilow and the two Pielsticker patents?

A. As a technical man, I think that they all describe and fulfill this process which you have described, and all equally well.

The Master: It is not enough that all of them describe it, but it is required that some one describe it.

The Witness: Laing, the first reference I gave, describes it without uncertainty, and in view of condensing under pressure, Pielsticker's British and Pielsticker's United States—in view of condensing under pressure being old, the Pielsticker British and the Pielsticker United States and Schuchow and Gavrilow do describe it thoroughly, as also does the Dewar and Redwood and the Redwood citation.

The Master: There must be continuous supply in the first place; there must be pressure, in the second place; and in the third place, the pressure must continue through the system, including the condenser. Those are the three requirements, are they not?

The Witness: Yes.

The Master: Are they all met with in any one of those patents?

The Witness: They are all met with; Laing meets them, Dewar and Redwood meets them, and the Redwood citation, "Petitioner's Exhibit 107." And if we take, which was true, that condensing under pressure was well known and could be added to, and was not directly the purpose of these three other patents, Pielsticker British, Pielsticker United States and Schuchow and Gavrilow, we have those in addition. But the first three include condensing under pressure.

The Witness: Laing actually includes condensing under pressure.

[fol. 755] Q. As between Laing and Dewar and Redwood, or Redwood, I will press my former question, as to whether Laing is any different in kind, with reference to the subject of this claim 1 of the Adams patent No. 1,327,263, from Dewar and Redwood or Redwood?

A. Yes, inasmuch as he describes a different apparatus, and to that extent he is different, from a technical viewpoint, these are all equally good descriptions, and each has a perfect description of the process which you have described.

Q. Now, I am still pressing my question. Do you assert there is any difference in kind——

A. In what?

Q. In kind, or only a difference in the form of apparatus which you say was used to carry out the process as between the Laing article on the one hand, and Dewar and Redwood or the Redwood article on the other; with reference, of course, to this process of Claim 1 of the Adams Patent No. 1,327,263.

A. Laing describes his process definitely as a continuous process, using the word "continuously." Dewar and Redwood described it in a form, page 2, line 21:

"By another pipe, E-2, furnished with a cock or valve, the contents of the retort can be drawn off, or this pipe may communicate with a second retort which in like manner may connect with a third so as to permit a series of any required number."

[fol. 756] This is an indirect description of a continuous process.

Q. Now, read from Laing without any of your own valuable comments thereon, what you rely upon in saying that he describes the process.

A. It is essentially the whole article——

Q. No, I am asking you to read from the article without any comments of your own.

A. It is essentially the whole article, but in particular pages 157 and 158.

Mr. Cooper: I move to strike that answer, if your Honor please.

The Master: Your motion is overruled.

The Witness: Now, I am reading from——

The Master: It is not necessary to read it into the record again; it has been put in the record.

Mr. Cooper: What?

The Master: It has been in the record several times. The witness, in his answer to the question, gives a specific direction to look for it on certain pages. If the Court can not find it there, why, it is not there, and that is all.

Q. Read from those pages, or any part of the article, where it describes that the operation, whatever it was, of Laing is continuous.

A. Page 157, line 4 from the bottom:

"The still can be fed continuously and I have made provision for the removal of residues in their most valuable form."

Q. In that passage is not Laing referring to an atmospheric pressure still, and not one that is operated under pressure?

A. But he says there that it can be operated under pressure. On page 158, and on 159 he states that he can place the pressure valve at the end of the condenser coil, so that you have a joint method here—an alternative method of constructing and of operating this and the disclosure is, accordingly, a complete one.

Q. Now, do you not argue your case. Let the Government's attorney do that. Answer my question. In the passage which you cited from Laing, is he not there describing an atmospheric process and not one under pressure?

[fol. 757] A. No, because he is describing the same apparatus which he describes in his United States patent, No. 741,291, of March 22nd, 1892, in which he states that he uses a pressure of 80 pounds per square inch.

Q. Where does he state that, in the United States patent?

A. Yes.

Q. You now introduce the United States patent as a satisfactory reference under this claim 1 of the Adams patent, No. 1,327,263?

A. It is another description or another article by Laing, and the three articles by Laing are connected, but this is simply used in interpretation of page 157 of Laing.

Q. Now, are you not mistaken in your reference to the United States patent to Laing. Identify it by number.

A. No. 471,291, and I refer to line 79, where it speaks of 80 pounds per square inch.

Q. Now, do you introduce that U. S. patent of Laing as one that discloses the process of Claim 1, of the Adams patent, No. 1,327,263?

A. No. What I introduced was the Laing article and the description here of a process which is described in there, particularly pages 157, 158, and 159. You raised the question as to the character of the still described on page 157, and I quoted back to the earlier written application of Laing.

Q. Now, where did he say in the article that he is referring there to his United States patent which you now cite?

A. He does not refer to it by number or a citation of the United States patent. In this same article he gave an illustration which is essentially the same as in the United States patent—a drawing which is essentially the same, though takes a somewhat different form from that in the United States drawing.

Q. Now, are you not entirely mistaken about that? Is it not a fact that the illustration which he gives at Fig. 1 of the Laing article, is the other United States patent of Laing, to-wit, No. 488,767?

A. And Plate 3 is essentially the same as the United States patent, not Fig. 1.

Q. But that part of his article does not refer to the Plate 3, at all, does it? It is Plate 1, is it not?

A. If you will note also——

Q. Answer my question; do not argue the case.

The Master: Answer yes or no. Begin your answers with a "yes", or "no", then if you want to explain it, you can do so.

A. Possibly, but it is not clear as to whether it is to Plate 1 or Plate 3. And as a matter of fact, Plate 3 is but covering

a supplementary apparatus to go to condense the vapors from the Fig. 2 in Plate 1—Fig. 2 not showing any condenser—Fig. 2 of Plate 1, not showing any condenser; the Plate 3 showing the condensing apparatus corresponding to it.

Q. That is to say, with reference to Plate 3 of Laing, the part on the left hand side, identified by the letters of reference, A, is intended to represent the part marked D4 of Plate 1 of the Laing article?

A. Yes, but it does not have even the supplementary parts on it as shown on Fig. 2 of Plate 1.

Q. Now, in such a process as that defined in Adams claim 1, of patent, No. 1,327,263, when conducted with apparatus like that shown in the sheets of Adams' drawing, state your opinion as to whether or not it is advantageous to avoid highly heating the chamber where the bulk of the carbon accumulates?

[fol. 758] A. It is.

Q. With reference to the Schuchow & Gavrilow Russian patent, which part of the coil, dd, do you assert to be locally heated?

A. In particular the portions near the fire box to the left; that is, the lower portions there would receive more heat than the upper portions of the coils, dd'.

Q. Now, assume that that erection there shown is ten feet high, that is, it is ten feet over all height up to the top of the dome or whatever the thing is above the member marked A, hoy many feet of the length of the coil shown there do you say are locally heated in the sense of your last answer?

A. All of the coils are heated, but the heating is applied in greatest intensity, and in that sense, having no special local heating.

Q. Well, how many feet?

A. The lowest portion would receive most, but I should say, roughly, if that apparatus was ten feet high, we would have the lower six inches of the vertical height of the coils, and perhaps the upper six inches, as the portions which were receiving too little heat to be spoken of as "local heating." "Local", is simply a comparative term, and we would have, presumably, cracking through essentially the whole length, excepting the bottom part where it has not yet reached the temperature, but which is being rapidly heated.

Q. And those pipes, on the assumption of the dimensions in my question, since the pipes travel back and forth through

there, they would be something like 150 feet long, would they not, that are locally heated?

A. No.

Q. Now, take up Claim 9 of this Adams patent, No. 1,327,263, and state where you find in a single reference in the prior art, the disclosure of a process of converting oil, which consists in continuously passing oil in a relatively thin stream under pressure, through a pipe-like heater to a larger container or vapor dome, in open communication therewith, whereby the pressure will be substantially the same in said heater and dome, heating said oil locally as it passes through said pipe-like heater to a cracking temperature, passing the vapors from said dome through connections to a condenser [fol. 759] all of the connections to and through the condenser being in open communication so as to insure a substantially uniform pressure from the heater through the dome and condenser. By "prior art" I refer to anything prior to January 1st, 1910.

A. Petitioner's Exhibit 135, Lang United States patent, No. 954,575. Of the citations that I have at hand, this is the only one which gives it in a single reference. There are others which give it in the combination reference, such as Pielsticker.

Mr. Cooper: I will object to this.

A. (continued). In view of Lang——

Q. Now, this Lang patent to which you refer as the only one that is in your judgment self-sufficient, you had your attention called to the fact, have you not, that that was cited as a reference in the Patent Office and the claims were nevertheless allowed?

A. I did not recall that this was cited by the Patent Office, and did not have it in mind; if that was correct, that it was cited.

Mr. Rauber: Before proceeding with the redirect examination I wish to put in a little further direct examination.

The Petitioner now offers the following additional publications:

As Exhibit No. 245, the title page and pages 29, 30, 31, 35, 48 and 127 of a book by Edmund J. Mills, entitled "Destructive Distillation" published at London in 1892.

As Exhibit No. 246, an article by Dr. Krey, entitled "Die Mineralol-und Paraffin-Fabriken der A. Riebeck'schen Mon-

tan-Werke, Actien-Gesellshaft," published in 1889. The translation accompanies that. The particular pages are, the title page and the introductory page, and pages 9 to 14, inclusive.

As Exhibit No. 247, an article by Dr. Krey, in the Jahresbericht des Techniker-Vereins der sashsisch-thuringischen Mineralol-Industrie fur 1887," in the mineral oil industry for 1887, printed 1888) and comprising the title page and pages 17 to 23.

As Petitioner's Exhibit No. 248, the title page and pages 88 to 90, inclusive of Dingler's Polytechnischers Journal, Volume 266 of the year 1888, comprising an article marked D in these pages.

And as Exhibit No. 249, "Inaugural-Dissertation," by Josef Jacunski, Part 2, der mathematisch-naturwissensachaftlichen Fakultät. That is the year 1898.

I have one more publication I wish to offer as an exhibit; it is Inaugural-Dissertation by Hert Grüning, Die Zersetzung schwerer Mineralole beim Erhitzen, published in 1898, pages 8 to 17, inclusive, and the title page and introductory page.

[fol. 760] Mr. Wiles: The Dingler Exhibit, "No. 248" we will agree to, of course. We make no objection as to that.

The Master: "Exhibit No. 248" is admitted in evidence.

(The said "Exhibit No. 248," so offered, was admitted in evidence, and the same is herewith returned.)

Mr. Wiles: "Exhibit No. 245" is all right, and we have no objection to it.

The Master: "Well, then, "Exhibit No. 245" is admitted in evidence.

(The said "Exhibit No. 245," so offered, was admitted in evidence, and the same is herewith returned.)

The Master: Let the record show that "Exhibits Nos. 246, 247, 249 and 250" are admitted in evidence.

"Exhibits Nos. 245 and 248" were admitted yesterday.

The said "Exhibits Nos. 246, 247, 249 and 250," so offered, were admitted in evidence, and the same are herewith returned.

Further direct examination.

Questions by Mr. Rauber:

Q. On page 8 of my typewritten copy of the translation of the article marked "Exhibit #246," the 14th paragraph, I find mention of 66 degrees Baume, and 50 degrees Baume sulphuric acid—what concentration would that be?

A. 66 degree Baume, sulphuric acid, is approximately ninety-three per cent. 50 degree Baume sulphuric acid, is approximately sixty-two per cent.

Q. On page 5 of my typewritten copy of the same article, at about the ninth paragraph, I find a statement referring to the stills of the Riebeck Company—that the stills for the distillation of the light oils are provided, for the purpose of a sharper separation of the distillates with columns—of two meters height—similar to those used in the apparatus for spirit distillation.

Would you understand that to be a bubble tower or fractionating column?

A. Yes, it is such, and by spirit is meant alcohol distillation.

Q. On page 3 of the article, "Exhibit No. 247," I find the number 500 kg.—how many pounds would that be?

A. 500 kg. means 500 kilograms, and would be 1100 pounds, approximately.

Q. On page 10 of my translation of "Exhibit No. 247," there is a statement that the distillate is distilled up to 190 degrees. If that distillate were purified in the manner described as being used in this plant, would it correspond with, or be a motor spirit, or gasoline?

A. 190 degrees, here, is 190 degrees Centigrade, and such product if purified by the customary method, with caustic soda, and so forth, would be a motor spirit.

Q. In "Petitioner's Exhibit No. 249," do you find any description of heating under pressure and digesting above 250 degrees Centigrade?

A. Yes.

Q. Do you have any idea what the pressure- were in that digestion?

A. The pressures were decidedly above four atmospheres and may have been as high as twenty atmospheres.

Q. Do you understand that the product was subsequently distilled?

A. It was.

Redirect examination.

Questions by Mr. Rauber:

Q. In cross-examination, Doctor, you described your qualifications up to the time you became a teacher at Columbia. What rank do you hold in the Faculty of Columbia?

A. Professor of Chemical Engineering in the Faculty of Applied Science.

Q. Is there any position of higher rank in that Faculty of Applied Science?

A. Not in the teaching division. There is such a position as a Dean.

[fol. 761] Q. Doctor McKee, you said, as I understood you, in your cross-examination, that bubble towers, or fractionating columns, ought to have some dephleg*a*mator or cooling means. Assuming that huch a column were exposed to radiation effect, would such a dephleg*a*mator be absolutely essential—is that your position?

A. No, but it is better design to use a dephleg*a*mator rather than to use a column in which simple radiation takes the place of the dephleg*a*mator, in bringing about a condensation of the liquid, and the return of the liquid to the base of the column, thereby washing the vapors.

Q. Now, why is that preferable?

A. Because if you do it by radiation, you have radiation taking place particularly at the lower portions rather than at the upper portions, the lower portions being at a higher temperature than the upper portions of the tower, and we would have, then, that the column effect, which is dependent upon the return flow of the liquid, would be only really effective in the lower part of the column, and only in a minor way at the upper part of the column—whereas, if, instead of that, we use a dephleg*a*mator and insulate our column, we have a return of the same amount of liquid from the top of the column, going through the entire length of the column, and thereby becoming much more effective—which means a saving of fuel, an increase of capacity, and permitting the use of a smaller column.

Q. As I recall, you stated that Pennsylvania oils contain some sulphur. Can you give us an idea as to how much sulphur they contain?

A. A tenth of a per cent, or less.

Q. And is that what you would call a trace?

A. Yes.

Q. Just a mere trace?

A. Yes.

Q. Considering the matter from the standpoint of the purchaser, what is gasoline—briefly?

A. We have a small amount of gasoline which is used for the making of gas by bubbling air through it, but the larger amount of it is used, essentially as motor spirit, and gasoline may be considered from the viewpoint of the purchaser, in general, to be a material obtained from petroleum, either by straight distillation, or by cracking, or by blending of either of these with casinghead gasoline—which is suitable for use in an automobile engine.

Q. Do you include in those engines, aeroplane engines, too?

A. Yes, certainly.

Q. What is a motor spirit?

A. A motor spirit is a broader term, which includes more than gasoline. It would include all types of gasoline suitable for use in internal combustion motors, and it would, in addition, include such materials as benzol, as mixtures of benzol with parts of gasoline, of gasoline with alcohol, and gasoline, or alcohol with ether, and, possibly, even alcohol, alone—and by alcohol alone, I am referring essen- [fol. 762] tially to denatured alcohol—and, possibly, other materials which may be used. It is a broader term than gasoline.

Q. By gasoline you mean any of that material that might be used in aeroplane or gasoline motors, or automobile motors, or for making gas by bubbling air through it?

A. All of those are included in gasoline.

Q. Would they be included in motor spirits, too?

A. No—that is, the gasoline that is used for making gas would not be, from the purchaser's side, a motor spirit. It is not used for that purpose. But, on the other hand, it is suitable for use for that purpose.

Q. If you were purchasing a motor spirit, say, in Scotland, near a shale oil plant, would you be apt to get shale oil gasoline, or other kind of gasoline—what would you get?

Mr. Wiles: I object to that.

The Master: I think that is——

Mr. Rauber: I will re-word it.

Q. If you were purchasing motor spirit in Scotland, what would you be apt to get?

Mr. Wiles: I object to that.

The Master: He might get shell oil, in Scotland. The Shell Company is selling motor spirits all over Scotland. The objection is sustained.

Q. If you were to purchase gasoline, could you tell whether it was a shale oil gasoline, or common, ordinary gasoline?

Mr. Wiles: That is objected to as too indefinite, and if he says "can you tell other gasoline from regular gasoline," that is another question.

The Master: That is what I understand the question to be.

Mr. Wiles: Then why the hypothesis at the beginning?

The Master: He may answer the question.

A. The two would act essentially the same in a car, and I don't think that the use of it by the purchaser would show any difference, so that he would be able to tell the difference.

Q. During your cross-examination you produced a sample of shale gasoline—do you have any water-white samples of shale gasoline?

A. I have this sample, which is a sample which I produced in my cross-examination.

Q. The same sample?

A. Yes.

Q. How do you account for the difference in color—if there is any difference? This is the water white, is it not?

[fol. 763] A. This is the second sample I produced—not the first sample—and it is lighter in color, because it has had more complete chemical treatment.

Q. Do you know the iodine number of this sample?

A. Yes. It is 76 and a half.

Q. By what method?

A. By the Hanus method.

Q. Is this gasoline produced from shale by the ordinary method without pressure distillation?

A. It is produced from shale, without pressure distillation. The method is a variation of the ordinary method.

Q. But it is substantially the same as the ordinary method?

A. Well, substantially the same, yes.

Mr. Rauber: I offer this sample in evidence as "Petitioner's Exhibit No. 251."

The Master: It may be marked and identified, and it has been sealed, and it may be retained in the possession of the Petitioner as a physical exhibit.

(The said glass bottle, so offered in evidence, was marked "Exhibit No. 251," as a physical exhibit, and was retained by counsel for the Petitioner.)

Q. Doctor, is this, in your opinion, substantially the same as the shale gasoline produced prior to 1910?

A. Yes.

Q. Do you know about what the boiling point range of this gasoline is?

A. I gave it in my cross-examination, but I don't recall it now, and I don't have the memorandum here. Possibly I have. Yes, I have it here. It is, from approximately 200 degrees Fahrenheit, as the beginning, to an end point of 360 degrees Fahrenheit.

Q. In "Petitioner's Exhibit No. 198," the article in the Berichte, there is an analysis showing that the product of the Riebeck process contains a number of boiling point fractions—or, rather, constituents, boiling over a range of from 100 to 200 degrees Centigrade, or more.

Knowing that the raw material has constituents of this boiling point, would the ordinary chemist know how to make gasoline from it?

A. Yes, if those constituents were hydrocarbons.

Q. How does that compare with the ordinary method of taking gasoline from crude oil?

A. It is simply a more exact separation of it by boiling point.

[fol. 764] Q. You didn't quite understand my question. I mean, the process that the chemist would subject the

product to, analyzed by Engler—how does it compare with the ordinary method of producing gasoline from crude oil?

A. It would be essentially the same. He would simply distill off the portions between certain ranges, up to say 400 degrees Fahrenheit.

Q. Would the same hold true of the product described by Thorpe and Young, in the proceedings of the Royal Society?

A. Yes.

Q. If you had these various fractions made by Young, or Engler, as described in these exhibits, which I have just mentioned, would the ordinary chemist know how to put them together to make gasoline?

A. Yes, he would simply pour them together and mix them, and they would be a product useful for a motor spirit, and of the same character as we commonly use now.

Q. On page 19 of your book on "Shale Oil," you mention the term "New cracking method." Will you explain what you had in mind when you wrote that phrase?

A. I was referring, here, to the fact that the percentages of gasoline obtained from crude petroleum had been lately considerably increased by these methods, and I was referring to the fact, that, whereas, in times past they were using methods for getting gasoline which were atmospheric methods—whereas, in the present time, they were using methods on large scale, which had increased this yield of gasoline, and that these were large commercial outputs, and these are methods of pressure type, in the present day.

Q. Have you any further explanation of your statement, to make?

A. As I have stated, in my cross-examination, I knew that the methods, here, which were used, were not scientifically new, but I thought of them as new in this country, in large commercial production. They were not either scientifically, or technically new. The investigation that I made was, as I stated in my cross-examination, about 1923.

Q. In the description of the iodine numbers of various hydrocarbons, you found certain anomalies in the case of the aromatic compounds, did you not?

A. I did not determine the iodine number on any pure compounds excepting benzol. That was the only one which was strictly within the field that you have mentioned—that

is, as a single, individual, compound. While benzol is known as an unsaturated product, it is also well known to react with bromine and iodine, with considerable difficulty, and, indeed, scarcely at all.

[fol. 765] Q. Does that hold true with any of the straight chain hydrocarbons?

A. Yes, of the saturated ones, not with the unsaturated ones. That is, the saturated ones react with great difficulty, with bromine, and practically not at all, with iodine. But the unsaturated ones react almost instantly with bromine, or iodine.

Q. Is the amount of absorption of iodine and bromine substantially proportional to the unsaturatedness in straight chain hydrocarbons?

A. Yes.

Q. Referring to the French patent to Kittle—in the case of the tank into which the heating coil empties—if this tank were used only for separation of vapors and liquids, would it make any difference whether it were inside of the lead bath, or not?

A. It would make no particular difference.

Q. If the tank were used for digestion, would it make any difference whether it were inside or outside the lead bath?

A. It would make a very considerable difference in that case, because, if outside, it would not have as high a temperature.

Q. On page 1447 of the typewritten transcript, in one of the questions, there, I find this statement:

"Now, the presence, therefore, of a very small quantity of quickly evaporating material, would greatly increase the volume of this foam column, and the volume of the vapors and accelerate the velocity all the way through, won't it?

Would this acceleration be of any advantage in cracking coils, in the cracking process?

A. May I ask, is that in connection with the Schuchow & Gavrilow process?

Q. Yes.

A. Yes, it would be a distinct advantage, in that it would tend to blow out, by the high velocity, any carbon.

Q. In the case of the twine oil patent—the viscosity is stated in the terms of Saybolt viscosities. Is this the stand-

ard that had been used in the literature and in the art prior to, say, 1910?

A. No. Engler was the commonly used standard viscosimeter.

The Master: That was thoroughly gone into and he explained the difference between Saybolt and Engler.

Q. Referring to the Pielsticker patents—would you [fol. 766] understand that the drum into which the coil empties, is at a greater or less, or at the same temperature as the coils?

A. At approximately the same temperature, though, perhaps, it has somewhat less temperature, because it is a larger apparatus, being heated essentially, by the same size furnace, judging from the drawings.

Q. In specifying what temperatures are to be used in various processes, is it generally understood or not whether there is some latitude allowed?

Mr. Wiles: I do not think that question is competent.

Mr. Rauber: I put it in the alternative.

Mr. Wiles: I am not objecting to the form, but I think it is perfectly incompetent in that general fashion. Each reference, for that matter, must stand by itself, it seems to me. Some processes may have some latitude, and some processes may not have any. I do not think you can have a general understanding as to all temperatures being subject to variation or how much.

The Master: I do not know what his answer is going to be. Let us hear his answer.

A. Such temperatures are ordinarily given and the meaning is that they are approximate; in fact, various parts of the apparatus will naturally vary in temperature to a considerable extent, if the apparatus is at all large.

Q. Would a pyrometer placed at one point in a retort or still indicate with absolute accuracy the temperature throughout the still or retort?

A. No, there would be variations, normally, within the still or retort, very considerable variations.

The Master: You went all over that, particularly with reference to the lead bath in Kittle.

Q. If you found resinifying effect upon exposure to air, what constituent of the air would you attribute it to?

A. The oxygen. The nitrogen and the other constituents are practically inert in chemical reactions.

Q. In the French patent to Vittinet, the use of an acid solution of bichromate is mentioned; would this have the same effect as air?

A. It would be an oxidizing solution, and give, even stronger oxidizing action than air. The degree of oxidation would depend upon the amount of material to be oxidized, and also upon the amount of the oxidizing agent present. Vittinet only has present a quite small amount.

[fol. 767] Q. Would its action be more rapid than that of the air?

A. Yes, it would be expected to be more rapid.

Q. But if the amount of bichromate were limited, as in Vittinet, would the action of the bichromate be limited also?

A. Yes.

The Master: You mean the oxidizing action of the bichromate would be limited?

The Witness: That is correct, but that is practically the only action we ever get from a bichromate solution.

Recross examination.

By Mr. Wiles:

Q. Referring to Exhibit 246, you were asked a question about the bubble tower. Those are described as being on atmospheric pressure stills, are they not?

A. Yes, as I understand it.

Q. Now, you stated that the Exhibit No. 249, at least I understood you, and I want to be perfectly clear that I have my understanding right that Exhibit 249, just offered, shows digestion without simultaneous distillation; is that correct?

A. There is distillation but it is from one part of the hot apparatus to another part of the hot apparatus; the part which is distilled is still at a temperature of 250 degrees. It is afterwards taken out and distilled at atmospheric pressure.

Q. The fact is, is it not, that the distillation was into a part which was at 150 Centigrade, not 250, as you state?

A. No, at page 6, I read:

"The experiment is now changed in such a manner that the leg which contains the paraffin was heated to 500 degrees Centigrade, the empty one at 250 degrees Centigrade; therefore it was shown with that lapse of time, after twenty-four hours, heating had already progressed considerably further as compared with the former experiment."

Q. In other words, they first distilled this oil—there were two experiments, were there?

A. Yes.

[fol. 768] Q. And in the first one, the paraffin was distilled at 350 into a receiver at 150, is that right? That is on page 5.

A. Yes and no. It was distilled there, but there was more than distillation; it was kept hot.

Q. Well, it was distilled into a receiver that was kept at 150 degrees Centigrade; that is right, is it not?

A. That is correct.

Q. Then, in the second experiment, it was distilled at 500 degrees into a receiver at 250 degrees?

A. I do not think that the term "distilling" there is the proper word to use in describing your experiment. The paraffin was heated at 500 and decomposed, and after twenty-four hours there was distillate formed in the 250 degree receiver. In other words, it was a digestion process, and the starting material and the product were at all times at 250 or 500 degrees.

Q. This receiver, or the cooler, that is, that was at 250 degrees or 480 degrees Fahrenheit, was in open communication at all times with the hot leg at 500 degrees or 930 degrees Fahrenheit; that is right, is it not?

A. That is right.

Q. And therefore, as fast as anything tended to go over, it would freely go over and condense on the other side, during the entire twenty-four hours that the process took?

A. It might go over and condense at a temperature of 250.

Q. It would have to, would it not?

A. Apparently it did not, because it took quite awhile to carry through, something like twenty-four hours.

Q. It is not a case of "might", physical laws apply there, do they not?

A. But it took roughly twenty-four hours to carry through the digestion and transfer.

Q. Now, as a matter of fact, of physical law and physical necessity, as soon as any material was formed, which would condense at 250 degrees, it would go over to the other side and condense there, would it not?

A. No, it might stay in the 500 degree one, if it would condense at 250 degrees or above 250 degrees; in fact, it might be such as would not distill even at 500 degrees.

Q. Well, any product which would boil at 500 degrees or below, and above 250 degrees—that is, any product whose boiling point was between 250 degrees and 500 degrees [fol. 769] Centigrade or 482 degrees and 930 degrees Fahrenheit, would go over just as fast as it was formed, would it not?

A. It would go over, but I do not think we should say it would go over as fast as it was formed.

Q. There was nothing to prevent it from going over just as quick as it was formed, is there? There is no valve, no closed receptacle, nothing of that kind in that apparatus, is there?

A. The digestion chamber is a long tube in this particular phase——

Q. How long?

A. Ninety centimeters, I believe. I will find it here in a moment, I should judge from the succeeding page, where it speaks of the same kind of experiments, and states that the tube was ninety centimeters long; that is approximately thirty-six inches, that it would be ninety centimeters.

Q. And that included both the hot leg and the cold leg, the entire tube?

A. Yes, included the two tube ends, but neither of them is cool or cold.

Q. The hot leg and the cooler one, then?

A. Yes.

Q. So they would be about eighteen inches long, apiece, something like that?

The Master: I do not think you can carry over these experiments. They are separate experiments; and I do not think you can carry over dimensions from one to the other, can you?

The Witness: I would say that you could, your Honor, in this particular case, because here he calls attention that

it is a two-leg soda glass tube in both cases, and the kind of tube that would be used would be what we would normally call "combustion tubing for such bomb furnaces." It is a standard apparatus, a tubing which is about seven-eighths of an inch in diameter externally, and that is the commonest kind of tubing to use in such bomb furnaces.

The Master: About how long is 90 Centimeters?

The Witness: About 36 inches, 35½; about 36 inches.

Q. Half of that would be the hotter leg and the other half would be the colder leg?

A. Yes.

Q. Now, in the first experiment it is perfectly manifest from the description, is it not, that during the twenty hours of operation, the entire original charge distilled over?

[fol. 770] A. Which experiment do you speak of as the "first" Page 5, 6 or 7?

Q. The one at 350 degrees Centigrade with the empty leg at 150 degrees.

The Master: What page are you reading from?

Mr. Wiles: That is on page 5. It says, "After 20 hours the paraffin had distilled over:"

A. That is correct, except for the very small quantity of coke-like residues.

Q. Yes. And during that experiment, apparently the distillation was continuous during that 20 hours, the product was moving over gradually from one leg to the other?

A. But with decomposition.

Q. Yes, of course.

A. (continued): Cracking.

Q. Now, in the second experiment, where the temperatures were raised, the conditions were otherwise precisely the same, were they not? The physical conditions, the structure of the apparatus and all that?

A. I think so; the structure of the apparatus was the same.

Q. Yes. And there was a brown distillation product obtained in that second operation, was there not?

A. Yes, brown semi-liquid mass.

Q. Yes. And that went over during 24 hours?

A. Yes.

Q. Now, in your re-direct examination you were asked about the sulphur content of Pennsylvania oil, and you

said that a tenth of a per cent, or less, was a trace. What kind of oil were you talking about?

A. I was speaking of the Pennsylvania oil as given as typical oils by the Bulletin No. 17, Kansas City Testing Laboratory, where it gives six Pennsylvania oils, and the maximum content a tenth of one per cent.

Q. I did not mean where did you find the tenth, but you said to counsel, in answer to a question, that you considered that a trace. Now, a trace for what sort of oils, is that; in what sense do you say that a tenth of a per cent is a trace?

A. In the sense that it is a very small amount. We have other oils that will run 2, 3 per cent, and we have some petroleums that run as high as 6 and 8 per cent.

Q. You mean that practically in any oil a tenth of a per cent is insignificant?

A. No, it has a meaning there and it must be taken into [fol. 771] consideration when working with the oil, but it is small, very small, as compared with other cases where we find large percentages of sulphur.

Q. And is that equally true, that you would consider it a mere trace in oil products?

A. We were speaking of crude oils. I think it is a trace in crude oils.

Q. That is not what I am now trying to find out.

A. (continued): A trace in crude oils. It might be objectionable if it was present in the refined oil in a form that was chemically active.

Q. Then in some oils that might be much more than a trace, practically?

A. Well, I would think of it, say, in a refined gasoline as more than a trace, but in crude oil I would think of it as only a trace, because only a small portion of it would get into the gasoline.

Q. And in finished products a tenth would be highly objectionable and a very large quantity, would it not?

A. It might or might not be. I would not think it would be objectionable, for example, in a twine oil.

Q. How about gasoline?

A. Well, I spoke of it a moment ago as objectionable in gasoline.

Q. It is too much for gasoline?

A. Yes, in general.

Q. If you had a tenth of that amount or one-hundredths of 1 per cent, either as hydrogen sulphide, or as mercaptans, it would be extremely objectionable, would it not, in gasoline?

A. I do not recall just what the limit is for gasoline.

Q. The top limit is a tenth of a per cent, if you want to look it up; but I asked you a particular question as to particular forms.

A. I think we are near the limit, it would be extremely objectionable and it would be a very small amount. It would, however, be scarcely likely to be present, because gasoline is ordinarily given a so-called doctor treatment with a lead oxide dissolved in caustic soda, and such treatment would remove hydrogen sulphide completely, and probably the mercaptans approximately completely.

Q. In what forms does sulphur exist in oils, and in what forms is it objectionable in gasoline and why?

A. Sulphur exists—you mean in crude oils?

[fol. 772] Q. Gasolines.

A. In Gasoline?

Q. Before removal, of course; crude gasolines? In what forms is it objectionaleb in finished gasoline and why?

A. It is objectionable particularly in the forms of mercaptans and hydrogen sulphide and decidedly less objectionable in the form of thio-ethers, and the reason why it is objectionable is primarily due to corrosion defects rather than to odor, though all of these products do give some odor.

Q. And are those the only forms in which it tends to exist; that is, in which it is found and has to be removed to make gasoline unobjectionable?

A. No, there are cases where you would get elemental sulphur, either dissolved or interspersed in such products.

Q. Is that bad and why?

A. That will react—in the first place it will, on the evaporation, leave a residue, and in the second place it will serve, to some extent as a corrosive agent in it.

Q. And all of these forms are corrosive?

A. I do not think we have any evidence that our thio-ethers are corrosive.

Q. The mercaptans are?

A. Yes, to some extent.

Q. Now, you undertook in your redirect to give some definitions of motor fuel, motor gasoline and motor spirits.

Just tell us, please, all of the qualifications that a motor spirit has to meet for practical use—practical all around use in the cars, of 1912, and '13, if you will.

A. The definitions that I gave were, by the question, limited to the users' definition rather than a manufacturer's definition. Along this same line I would say that these types of motor spirit which have been mentioned must, in the first place, give easy starting; that is, they must have a reasonable amount of volatile material. They must have reasonable power; for example, it would hardly do to use a partially diluted alcohol because to do so we would have a reduction of power, and so little power that it might be impracticable to handle the machine in order to get reasonable speed or go up a hill. The motor spirit must be one which will be able to be handled by the carbureter of a particular machine, not only for an hour's time but for continued use. It, further, must be one which will not corrode the engine or its feed tanks or pipes.

I think that outlines the important points in such fuel.

[fol. 773] Q. Those are all that you have thought of?

A. I think those are the important ones. I have thought of others but they are relatively unimportant.

Q. Now, you stated there were some you had thought of that were unimportant; I want all you know about, please.

A. The question of the color of the gasoline is one which has been spoken of in times past, but in so far as its use to the purchaser is concerned, it is unimportant.

The question of odor of the gasoline is similarly unimportant, as far as the direct question is concerned.

The question, of course, of suspended matter in the gasoline is obvious, for to have suspended sand or clay or carbon in it would mean that it would clog our carbureters. Those things are obvious. They are not to be discussed as of any importance in this case because they are so obvious.

The Master: You are speaking now from a practical standpoint and not from the commercial standpoint?

The Witness: Yes, I am speaking from the user's standpoint.

Q. Now, have you given all the factors that you know of that affect a commercial motor spirit?

A. My question was not limited to the commercial motor spirit at the time; it was on the question of the user's.

Q. I want all the qualifications that have to be met for all-around use, or which have to be met for all-around use in 1912 or 1913.

A. You are broadening your question quite a little bit.

Q. For practical use, I am talking of; all-around use in a car.

[fol. 774] A. We would want other things which are, again, obvious: We would want the gasoline to be one that is properly refined and not have any residue of sulphuric acid in it, or suspended water or any other suspended material. It should be free from any considerable amount of gumming materials.

It would normally have not only a fairly low first distillation part, in other words, a low portion which would serve for the starting of the engine, which is of importance particularly in cold weather, but it would normally have, though not necessarily, an increase of boiling point for the distillation of the remaining portion.

In other words, the distillation figures, when plotted, would give a curve normally, instead of a straight line, though of course, that would not be true if we were using a pure compound, such as alcohol or benzol or ether.

Those, I think, are all I recall at the present; there may be others.

Q. You say this must be properly handled in a carbureter; just what do you mean by that?

A. Properly what?

Q. Must be properly handled in the carbureter. That was one of your first qualifications, "work properly in the carbureter." Now, what does it have to have to do that?

A. What I was referring to there—at that time I had not mentioned freedom from dirt, sulphur, sand, clay, suspended water and those things. If it had those in it would tend to clog the carbureter.

Q. And that is what you meant by "working properly in the carbureter"?

A. That is one, perhaps the principal thing. We mean that it must be able to be handled by whatever carbureter the machine is using.

Q. Now, how are you going to tell from examining the gasoline what makes it work properly in the carbureter?

[fol. 775] A. Well, gasoline that had suspended clay in it or suspended carbon or suspended water (water in some types of carbureters), any solid suspended in it would probably not work well in the carbureter, as gasoline that had suspended sulphuric acid in it would probably injure the carbureter and soon it would not work well in that carbureter, because the carbureter would have been injured.

Q. You did not have anything else in view when you used that broad expression "handle in the carbureter." You did not refer to its volatility in any sense?

A. I had spoken of volatility as separate from that and earlier.

Q. Yes. Well, you said it had to have these starting and power qualities, but you did not say anything that I recognized about volatility. If you mean to add something about volatility I would like to have your views on it.

A. I spoke of the volatility in more detail later when I spoke of the question of the distillation curve of the material, and not having too high an end point.

The Master: What is the difference between carburetion and volatility?

Q. Mr. Witness, what is the difference between carburetion and volatility?

A. The volatility is measured by distillation of it in a flask; the action in the carbureter is dependent upon volatility but it is measured by trial in that particular—whatever type of carbureter is used, and is dependent not only upon volatility but upon other factors, such as cleanness and freedom from suspended matter.

Q. Now, you stated here in answer to the question that I repeated from his Honor, that, in substance, the function of the carbureter was to volatilize and that the carburetion and volatilization, aside from dirt and the like, meant the same thing; now, is that true or not?

A. No, I should not say that that was true, and I did not so understand your question, if I replied that it was the [fol. 776] same. I think of gasoline as being largely liquid when it is forced as a spray from the carbureter into the machine itself. As I said, I am not particularly familiar with the various mechanisms of an automobile from a mechanical standpoint.

Q. Now, if it be the fact, as you say you picture it, that

a large part of the gasoline enters the engine in the form of a liquid spray, and not as a vapor, then what is the volatility requirement of the gasoline?

A. I am not certain that I understand your question. The ordinary volatility requirement is that, for example, as has been stated here several times, it shall boil below 437 degrees, I believe is the figure.

Q. Well, now, you did not put volatility into your requirements.

A. Yes, I did. I spoke of the question that an ordinary motor spirit would have, on distillation, not a straight line curve but a curve in which you have an end point of around 437 degrees.

Q. It must have that to be satisfactory?

A. Oh, no, not necessarily, but that while some of the pure compounds, such as alcohol or ether or benzol could be used there, and which have a flat distillation curve, ordinary motor spirits will have a curve as their distillations curve rather than a straight line.

Q. Now, do they have to have that? Does it make some difference in the operation of the car; if so, what is the difference? Why do we have a curve of distillation instead of a straight line?

A. Because if we started to use our low boiling products, such as the low ends there, and we get out an individual compound which would be low enough to serve as a starting material in winter time, or even say this time of the year, we would have to pay such a price for it that it would be beyond reason, and we try to put as much of a heavy in as our machine will stand. But my question was limited to the user's viewpoint, and with the user the question of price comes in as well.

Q. That is purely the question of price; so far as operating the car, it is your opinion that it might just as well have a straight line distillation, or a substantially straight line.

A. I know that there are internal combustion engines which have been operated on straight alcohol, or that al- [fol. 777] cohol is not quite a straight line curve; because it was not quite at the constant boiling mixture; but that would be a case of the use of a product having essentially a straight line, that is a single boiling point. I think, also,

that internal combustion engines have been operated on pure benzol, but of that I am not quite so certain. We ordinarily mix with it certain other materials.

Q. Well, it is wholly unsuitable because it will freeze in the winter, will it not?

A. It would be from that viewpoint.

Q. But you have not answered my question at all. Is there any reason, other than price, why we want a curved distillation running up to around 400 or 350 degrees, somewhere in that order; why we should not use one that runs along 225 degrees or 200 degrees?

A. It is a fact that——

Q. Can you not answer that yes or no? Is there any reason or not?

A. There is no particular reason, as I understand it, why we should not use one with a much lower end-point; in fact, our aviation, fighting grade gasoline is defined, I think, as one with something like 100 degrees lower end point than the figure that you have given of 437.

"96 per cent of the aviation, fighting grade gasoline, shall distill over at 235 degrees Fahrenheit."

So that that would be a quite low one and would be a useful gasoline for internal combustion engines.

Q. That is, suitable for all-around use in the automobiles of 1910, '11, '12, '13, as a motor fuel therefor?

A. Yes, I should say that it was, though somewhat expensive and somewhat volatile for use.

Q. I am leaving out price.

A. In hot weather there would be losses from it.

Q. No trouble except losses? It would run the car satisfactorily; a man could get up the grades and it would run with it?

A. I think that such high test gasoline would have been welcomed by many users.

Q. For the average, all-around user, is what I am getting at. Is that the kind of stuff we can sell in the ordinary service station, or could have sold in 1912, to one car after another as they came up?

[fol. 778] A. As I recall it that is, and in fact, in the State of Iowa it would pretty nearly have to be that; sold as gasoline, Iowa insisting that it must be a 70 to 80 degrees Baumé gasoline, and anything else is not legally gasoline,

and that it must be marked as "substitute for gasoline." I am referring particularly to the Bulletin 17 of the Cross laboratory.

Q. Where do you get this data on Iowa?

A. I read from the Bulletin 17, Kansas City Testing Laboratory, page 362:

"Iowa: Gasoline shall be between 70° Baumé and 80° Baumé and shall distill from 150° Fahrenheit to 210° Fahrenheit, all other portions shall be branded 'substitute for gasoline'; shall show percentage boiling below 135° F., from 135° F., or 210° F., or 210° F., to 302° F., percentage above 302- F."

Q. And it is your position, is it, that that Iowa gasoline, defined in the matter that you have just read, was a suitable motor fuel for all-around use in the cars of 1912?

A. I think it would be. It is a very high test gasoline but apparently the State of Iowa so legislates.

Q. Well, they can not make it suitable for a car; they may legislate about its name.

Now, I want unequivocally your statement either that it is or is not suitable fuel for all-around use in the cars of 1912.

A. It is my opinion that that would be a suitable gasoline for all-around use in the cars of 1912, though possibly not for an exceptional car.

Q. Now, why do we put an end point on gasoline, an upper end point; what difference does it make?

A. If you put in too much of the kerosene portion it would mean that there would be difficulty in starting the car. There would also be difficulty in volatilizing the product there, but it shows up particularly in the starting of the car.

Q. You can have plenty of light ends to offset your heavy, can you not, so it will start all right?

A. In other words, your thought is to make a mixture of——

Q. What is the matter with these casinghead, kerosene bootleg blends that get on the market every little while? Why do they not run all right, if they do not? Do you think they do?

I have no knowledge of them, sir.

Q. Have you given the only reasons that you know of for

[fol. 779] having an upper endpoint on our gasoline; that is, that it is the starting only?

A. I gave more than that.

Q. What are they?

A. On the question of the——

Q. I want all you know about.

A. If we use a heavy product in an internal combustion engine we will increase our carbon formation and we will get unburned oil from the engine also. I think, as I said, that these four things, the starting end, the question of the volatility, the question of the carbon formation, the question of unburned oil; those, I believe, are the four important things.

Q. And that is all you know about?

A. That is all I think of at the present instant.

Q. Now, you gave a number of motor spirits including alcohol and ether.

A. Yes.

Q. Do you think those are suitable for all-around use in the ordinary car of 1912?

A. A mixture of alcohol and ether has been used for a number of years in one section of the world as an automobile fuel, and with apparent complete satisfaction.

Q. Where?

A. In Natal, South Africa, and they use there the alcohol and ether made locally, instead of having to import from the United States or elsewhere.

Q. I do not care where they get it.

A. And also to a fair extent in Cuba.

Q. It is your position, is it, that that was suitable for use, for all-around use, in the cars of 1912?

A. I do not know whether they began it in 1912, but they were using it a few years later than that and it was practically the only fuel sold in that country and was satisfactory.

Q. Now, was it suitable for use in the ordinary car of 1912 in the United States of America; the cars running around the streets here?

A. I think so. It would be one, however, which would give increased detonation; in that respect it would give—it might give some trouble, particularly if it was a high compression car, but the cars in 1912, in general, were not high compression cars.

Q. And you do not know that about half of the cars which [fol. 780] were running around, it would not run at all, with any satisfaction?

A. I do not think that is so.

Q. If they would not you do not know why?

A. I do not think that is so.

Q. You brought in a sample of shale gasoline; when did you prepare that?

A. That was prepared November 10th and 11th; I think it was the 9th or 10th and 11th. It was finished on the 11th.

Q. During this examination?

A. Yes. It was the one that I brought in at the time that I was under cross-examination by yourself; this is the same sample.

Q. And it was just recently made, then?

A. Yes.

Q. Made after we measured the color of the other sample?

A. I think that we measured the color of the other sample about the day that I finished making this.

Q. Now, what textbook tells us that benzol is unsaturated; you say it is a well known fact.

A. Almost any textbook on organic chemistry, and I think I can locate it in some of these that I have here.

Q. Just name some of those, enough so you will be sure it is in there. I do not want to waste time having you hunt for it.

A. Bell's American Petroleum Refining, page 12, reads as follows:

"Benzene may serve as an illustration of the closed chain or ring compounds, its structure being as follows."

A formula is then given.

"Each carbon atom in this ring is united with a single hydrogen atom which may be replaced by other atoms or groups. The 'aromatic compounds' must contain this ring in some combination or other. When, as above, the valance of all of the carbon atoms of a compound is not completely satisfied by the atoms of hydrogen, the compound is 'unsaturated.'"

Q. Now, you talked of the advantage of the increased velocity in the Schuchow patent. Would you recommend putting benzine or light products into the charging stock of a cracking process?

A. It might be done. I would recommend it as a better procedure than the question of putting benzene or gasoline into the charging stock of a distillation process—a straight [fol. 781] distillation process. We normally would use, however, a cheaper material than benzene or gasoline. For this purpose we would normally use steam.

Q. Then you would not recommend these products for a cracking process?

A. I prefer steam, but they could be used.

Q. Now, you have said something about the Saybolt viscosimeter not being standard. It was, in fact, in 1913, the instrument used universally by the refiners who were doing over ninety per cent of the refining in the United States; that is true, is it not?

The Master: If you know you can answer yes or no.

A. I don't know what percentage of the refining was done by the Standard Oil Company at that date.

Q. How about 1911?

A. I do not know what amount of refining was done by the Standard Oil Company in 1911.

Q. You know, as a matter of fact, that the vast majority of the refiners in the United States, both in number, and in amount of products handled in 1913 did use the Saybolt viscosimeter, did you not; and did not use any other?

A. I did not know that.

Q. What is your opinion on it?

A. I have no knowledge.

The Master: If he does not know, that is a fact. If he does not know he need not give any opinion about it.

Q. Now, referring to the Pielsticker patent, you drew certain deductions from the illustrated size of the furnace in the two cases. I want to be quite clear that I have your position correctly: Did you mean that in the two furnaces there would be the same heat input to the oil, roughly, because the two furnaces are of the same size?

A. No. I called attention to the fact that in the larger chamber furnace there was more exposure and radiation,

and consequently, it would probably be cooler, that is, less heat input into the oil, though both were quite hot, and both would be somewhere—might easily be somewhere near, but probably, as I called attention in my redirect, the tank, I, in the British patent—the cracking tank, I, would be at a lower temperature due to the fact that it has greater radiation surface.

Q. How about heat input? Here are two furnaces of the same size. Would you not naturally assume, or is it your [fol. 782] position, that these two furnaces of the same size do not supply the same amount of heat?

A. No, I would say that they were in—so far as looking at them, could easily and would be perhaps expected to supply approximately the same amount of heat.

Q. They would be expected to?

A. Approximately so.

Q. That is what I thought. Now, when you previously have referred to certain per cents of unsaturateds in shale oil gasoline, as 40, 50 and 60 per cent, did you mean by the sulphuric acid absorption test?

A. Yes, in general. The sulphuric acid absorption test, as I called attention, is one which normally gives a somewhat low result.

Q. But you meant by that test.

A. But by that test, interpreted as to the amount of the unsaturateds that were present there. I called attention, for example, in my original direct work that the sulphuric acid test is likely to give us about five-sixths of the unsaturateds.

Q. Well, in some of your tests that you made, it gave higher results, did it not?

A. Yes, in one test.

Q. Now, when you say that an oil is fifty per cent unsaturated—shale oil gasoline is fifty per cent unsaturated, what do you mean by that?

A. If I said that, I would mean, in general, that it was composed of half its weight of saturated hydrocarbons, and the other half of unsaturated hydrocarbons.

Q. How much sulphuric absorption would you get on it?

A. Probably around—well, it would depend a little bit upon what kind of unsaturated hydrocarbons were present there, but take, say, a shale oil gasoline, you would prob-

ably get about—oh, 42. It would depend how much shaking you did, how thoroughly you did it, but carrying it as the ordinary Bureau of Mines test, you would probably get 42, 43 per cent.

Q. And you take the actual sulphuric acid absorption and swell it up or enlarge it a little, about a sixth, something like that, of its actual figure?

A. I think that would be an average figure. It is more common to speak of it as—well, for example, this gasoline, this one has an unsaturatedness, by sulphuric acid of 34 per cent. In other words, we ordinarily give our method when we speak of the unsaturatedness. Now, that does not include any addition of one-sixth to it.

By Mr. Neave:

Q. What do you mean by "this one"?

A. The sample of shale oil gasoline put in as Exhibit No. 251.

By Mr. Wiles:

Q. Then you would say, in all probability, that there was more nearly 40 per cent unsaturated than the sulphuric acid, 34; that is correct?

A. Probably.

[fol. 783] WILLIAM F. BESSELMAN, witness for the petitioner, formerly with the Valvoline Oil Co.

Cross-examination.

Questions by Mr. Wiles:

Q. Mr. Besselman, both in your direct and in your cross you made certain statements about the setting of the safety valve at a pound or a half a pound, and at one time you said you based that on reason rather than memory?

A. Yes.

Q. You, in fact, based that on the common practice of refiners, did you not?

A. Yes, sir.

Q. Now, you have worked in a great many plants during the many years you have been in this refining business, have you not?

A. Yes, sir.

Q. And you are familiar with the common practice in the oil industry of setting safety valves in ordinary stills that run at atmospheric pressures?

A. Yes, sir.

Q. And the ordinary and common practice as known to everybody in the industry is to set them at around two pounds or less, is it not?

A. Yes, about a pound. It depends upon the sized still and the strength of the still.

Q. But it would always be two pounds or less?

A. Less, yes, sir.

Q. And everybody that knows anything at all about this industry knows that fact, do they not?

A. Yes, sir.

[fol. 784] GEORGE B. GIFFORD, witness for the petitioner, formerly manager of two Standard Oil plants.

Cross-examination.

By Mr. Wiles:

Q. Now, you spoke of cracking always taking place more or less, and that in some processes you said you attempted to minimize the cracking, and in others you—in the kerosene cracking, you had formerly wanted to magnify it?

A. Yes.

Q. You had in mind the known process of the coking still when you spoke of kerosene, did you not?

A. I did.

Q. And you also had in mind the commonly called steam reduction, or steam refining processes, when you spoke of avoiding cracking in the manufacture of lubricants?

A. I did, yes.

Q. Now, your experience in oil refining, has been principally confined, has it not, to ordinary American petroleum?

A. Yes, to be sure.

Q. And you are quite familiar with various types of American—practically all the types of American petroleum and the usual refining processes that are practiced upon them?

A. Yes.

Q. Now, it is a fact, is it not, as you intimated, that cracking is extremely destructive of lubricants?

A. Absolutely destroys them.

Q. And that is true of all American petroleum oils that you know anything about, is it not?

A. Yes, sir.

Q. And a cracking process that would make lubricants is a totally and entirely different process from anything you ever heard of being practiced on American oils, is it not?

A. Yes.

Q. Now, when you practiced the process where you tried to magnify cracking in the manufacture of kerosene, that [fol. 785] practice found its highest development in the ordinary tower stills that were commonly used about 1910; that came in about 1909 and '10 to '13, somewhere?

A. Yes, those stills were brought in on account of the finding of a different grade of American oil.

Q. Yes. But that was the highest and final development of that type of cracking that you used in the making of kerosene?

A. Yes.

The Master: At that time?

Mr. Wiles: Yes.

The Witness: At that time.

Q. The tower still process finally became about the standard American method of refining—of the distillation of crude oil when you wanted to produce that kind of incidental cracking?

A. Yes.

Q. Now, in that process of cracking, as compared with the modern cracking to make gasolene, was extremely slight, was it not?

A. Yes.

Q. That is, you would not make more than two or three per cent. of modern gasolene fractions in the run of the still?

A. No.

Q. You might make quite a considerable additional amount of kerosene?

A. Yes.

Q. But the cracking was extremely small, compared to the process of today which will make 30 to 35 per cent. of modern gasolene?

A. Yes.

Q. Now, you have said that those processes made coke along with the cracking, and they did, did they not?

A. They did.

Q. They made, in fact, very large quantities of coke, did they not?

A. They did.

Q. Three and four and sometimes more per cent. of coke?

A. Yes.

Q. And the coke would be in a deposit that would run from, to be very conservative, 4 inches up?

A. Up to 22.

Q. Yes.

The Master: Now, when you speak of percentage you [fol. 786] mean three or four per cent. of coke in proportion to the weight of your oil?

Mr. Wiles: The oil charge.

The Master: The original oil?

Mr. Wiles: The original oil charge.

Q. And the deposit of coke in the bottom of the still at the end of the run would go from 4 up to, as you say, 22 inches, and it sometimes went deeper than that.

A. Yes. It wasn't much good to the still when it went deeper.

Q. Yes. Your heat transfer back was slow when it got deeper than that that it did not pay to run any longer?

A. Rather dangerous to the bottom of the apparatus.

Q. But in all of these operations, by the time you got anywhere near the end of the run, the bottom was very hot, was it not?

A. Yes, quite hot.

Q. It usually was red hot?

A. More than that.

Q. Yes. That was the normal course of the ordinary cracking operation that you have been testifying about, and that was common?

A. Yes.

Q. And the bottom from the mere weight of the material and the distortion of the iron, would bulge and swell and get all roughed up?

A. Yes, that is right.

Q. And with the mere weight of the oil you did not care how bad the bottom got, within any reasonable limits?

A. As long as it didn't split, yes.

Q. And it would stand an awful lot of bulging and bending and distortion without splitting?

A. It would.

Q. And in fact did.

A. Did.

Q. The conditions that I have been going over with you, Mr. Gifford, were those which were known, except for the detail of the tower still; they were known all the time in the oil industry and still are?

A. Yes.

The Master: What do you mean by "all the time"?

Q. Well, they existed from long before 1900?

A. Yes.

Q. In all operations and they still do?

A. Yes.

Q. In that same type of distillation operations?

A. Yes.

Q. They have been since long before 1900 and they still remain the standard, well-known conditions, in the oil industry that everybody in the industry understands and knows all about?

A. Yes, it is all current practice.

Q. Yes.

A. And every refiner knows it.

Q. Surely.

A. (continuing).—Has nothing to do with the case of cracking under pressure, other than in a general way.

[fol. 787] Q. So far as all the practice that I have been going over with you, with the exception of the details of the towers on the tower still, the practice that you and I have discussed has been perfectly common, perfectly well known from before 1900 down to date?

A. Yes.

The Master: That is what he said.

Mr. Wiles: Yes.

The Master: He had cleared that up.

Q. Now, the tower still that I spoke of was an improved detail of overhead that came in about 1909 or '10, just before the dissolution of the Standard Oil Company, did it not?

—. ——.

Q. It was commonly called the Van Dyke, or some people called it the Van Dyke still?

A. Yes, there were a number of them possibly at that time, although the tower had been used in distillations way beyond that period.

Q. You understood perfectly well what I meant by the tower still?

A. Yes, I understood what you meant.

Q. The one that everybody uses now?

A. Yes.

The Master: What you have referred to is what you have heretofore in the testimony referred to as a bubble tower?

Mr. Wiles: No.

Q. Now, the tower still involved that practice with the addition of a certain new type of overhead or dephlogmator equipment that went on to the still?

A. The tower still was nothing more nor less than the advancement of the art on the old air coil that had been used many years before. It simplified the air coil and brought it into a compact mass instead of spreading it all over creation as they did in the original process.

Q. Yes. It had to do with a detailed improvement in the overhead or runback equipment of the still?

A. That is all. That is the dephlegmation of oils to liquid, that is all.

Q. And that came in along about the time I have mentioned, 1908 or '09 or '10 or '11, somewhere?

A. Somewhere around there.

Q. And that type of modification became and has been since the common type of ordinary coking still?

A. In a refinery handling certain qualities or kinds of oils—classes of oils. That is so.

[fol. 788] Robert E. Wilson, a witness called on behalf of the Defendants, being first duly sworn, testified as follows:

Direct examination.

Questions by Mr. Wiles:

I am Assistant Director of Research for the Standard Oil Company of Indiana, having particular charge of research and development work on cracking, distillation and similar chemical engineering problems.

I took my undergraduate work in the College of Wooster, Ohio, specializing in Physics and chemistry, and graduated in 1914. I then took two years of advanced work in chemical engineering at the Massachusetts Institute of Technology, in Cambridge, Massachusetts. My first research experience was in the summer of 1915, in the Research Laboratory of Physical Chemistry at Technology, where I worked under the direction of Dr. A. A. Noyes. My next research work was in the laboratory of the General Electric Company, in Schnectady, New York, where I worked on some special problems for about six months, beginning in the spring of 1916, in chemical work.

In the fall of 1916 I returned to the Institute of Technology, as an instructor in chemical engineering, and a member of the staff of the Research Laboratory of Applied Chemistry where I worked on a variety of problems, some with a theoretical bearing, and some with an industrial bearing.

In the spring of 1917, very shortly after we entered the war, I was employed as a consulting chemical engineer by the Bureau of Mines, and had charge of a gradually increasing staff of men, who conducted investigations on various problems in connection with chemical warfare, particularly the perfecting of absorbents of protective clothing, screening smokes, incendiary bombs, and matters of that general nature.

This work was carried out at Pittsburgh, for the first several months, and in the fall we were transferred to Washington, and some time thereafter I was made a captain and later a major in the chemical warfare service still having charge of very much this general type of work, and I had the direction of upwards of fifty men engaged in these various lines of investigation.

Shortly after the end of the war, when this work was completed, I went back to the Institute of Technology, tak-

ing with me a number of my best men, and acted as assistant director, and later as director of the much enlarged Research Laboratory of Applied Chemistry at Technology.

We got contracts from a large number of industrial concerns, to do industrial and particularly chemical investigations. Our clients included the Goodyear Tire and Rubber Company, the General Motors Corporation, the United [fol. 789] States Steel Company, through one of their subsidiaries, and the Vacuum Oil Company, in general, taking only one company in a particular field.

About that time my interest in oil refining had its real inception, and we had three or four men, as a rule, working on problems, for the Vacuum Oil Company. I made a particular point to read up quite thoroughly the literature of the petroleum industry, and had an opportunity to visit several refineries, and also to investigate cracking processes, and other processes, some for the Vacuum Oil Company, but most of them for other companies.

We also did a great deal of work on the properties of motor fuel and on lubrication problems, for the General Motors Corporation, most of our work for them having a bearing on the different properties of gasoline, such as the volatility and the anti-knock tendencies of various gasolines.

I might add that toward the end of my stay at Technology I was made associate professor of chemical engineering.

In the fall of 1922 I went with the Standard Oil Company of Indiana, to take up, essentially, my present line of work.

In that work I have been afforded every opportunity for becoming thoroughly familiar with refining practice, not only in our own refinery, but in a number of other refineries, both of our Company and other companies. I have concentrated particularly on studies of distillation problems and cracking problems, because those are two of the most important things which the Company carries on, and in the course of that work I have made it a point to discuss a great deal of the past experimentations, and study some of the old reports on cracking, in order that we might not duplicate work that has previously been done, and I tried to find out exactly the reasons why different steps were taken, and what the results were from various cracking processes.

I have also been assigned to the problem of investigating a considerable number of other cracking processes, which have been brought to our company from various sources, to see if we might not be interested in adopting these. That has broadened my experience in the cracking art work.

In preparation for testifying in the case of the Universal Oil Products Company against the Standard Oil Company of Indiana, I had occasion to try out in actual runs on a semi-commercial scale a number of prior art cracking processes, including the Dewar & Redwood and the Laing and others.

[fol. 790] I might add that I have found that a view of the industry from within is quite different from that which one can read in all the books about the industry, because the industry seems to be rather unusual in its secretiveness, not deliberately, but simply that the men who have actually been engaged in the refining operations have not been writing books; and, whereas most of the publications are based largely upon European practice, there is a whole mass of American practice about which you can get little reliable information, at least until within very recent years. Within the last few years there have been some publications by the scientific men in the industry and they have intended to make somewhat more clear just what the problems are in the refining of petroleum.

Q. Mr. Wilson, you spoke of this work with cracking processes that you did in preparation for testimony in a certain suit. That is the so-called Dubbs case, is it not?

A. It is.

Q. And that work was done before this suit was started and quite independent of anything here?

A. Yes.

The Master: In that socalled Dubbs case you were representing a defendant?

A. The Witness: Yes.

The Master: As a defendant you were interested in exposing the prior art?

The Witness: Yes.

A. Crude petroleum, using the word in the American sense, as I shall ordinarily do in my testimony, is a naturally occurring liquid, flowing or pumped from the ground and it is composed mainly of hydrocarbons. It generally has along with it the hydrocarbons small amounts of sulphur

compounds, sometimes oxygen compounds, and rarely small amounts of nitrogen compounds.

Crude petroleum varies greatly in its properties in the different fields, some being very light in color and gravity, and containing a large amount of gasoline, and others being very black, viscous, heavy oils composed mainly of asphalt.

I have with me a sample of mid-continent crude oil which is representative of probably the most important class of crude oils in the country, which you may be interested in (handing sample to Master). We will formally offer it later.

Q. Now, are the hydrocarbons of which you have stated the crude is mainly composed, one type or of several types?

A. The hydrocarbons are of a number of different types.

Q. How are the crude oils generally classified in the industry?

A. The general classification of American crude oil is into three broad classes.

First, the socalled paraffin base oils; such as those from the Pennsylvania field, which contain considerable quantities of paraffin wax and little or no asphalt.

Second, the asphalt base oils, which contain little or no wax, and relatively large amounts of asphalt.

And, third, the asphalt mixed base oils, of which the mid-continent oil is a typical example, which contain both asphalt [fol. 791] and wax. The straight asphalt base oil generally, though not always, contain large proportions of napthenic hydrocarbons, and in such cases, particularly if the amount of asphalt is small, they are sometimes referred to as naphthene base oils; so that the terms "napthene base oils" and "asphalt base oils" overlap to a considerable extent.

There is another rough line of demarcation generally made between of heavy and light crudes in each of the different classes.

There is no hard and fast classification of the hydrocarbons present in petroleum, because there are such a tremendous number of them and we do not know exactly what ones are present. They probably contain several thousand different chemical constituents and the separation of them is a terrific problem.

You could give a good chemist good equipment and a year's time and as much of that oil as he wanted to find out just what was in it and he would be fortunate if, in a

year's time, he was able to identify—separate and identify with certainty ten or twelve of the constituents present. The rest could probably be classified into certain types and certain groups, but you couldn't swear what chemical constituents were present. It is entirely different, therefore, from analyzing salts, though they may be complex. Even sea water is a complex thing, but we can state exactly what are the products in it within a reasonable time.

The Master: Would these divisions aside from the ten or twelve be subdivisions of certain types?

The Witness: Well, the classification into different types of products would be entirely based on certain empirical tests. For example, one of the best tests for roughly dividing two general classes of hydrocarbons is the solubility, that is behaviour, towards concentrated sulphuric acid. The hydrocarbons which dissolve in or react with concentrated sulphuric acid are called unsaturated hydrocarbons, and the hydrocarbons which do not re-act with it are called saturated hydrocarbons. That is a typical method of analysis, you might say, of crude petroleum, yet it really tells us nothing about the exact constituents, or very little about the exact constituents.

The Master: Why saturated, why unsaturated? Why is that adjective used?

The Witness: The unsaturated hydrocarbons are pretty generally recognized as olefine-olefinic hydrocarbons, that is hydrocarbons which have a chain of carbon atoms with hydrogen atoms on it, such as plaintiff's expert sketched, but with double bonds between at least two carbon atoms. I can sketch this very well in the record.

[fol. 792] The fact of this double bond being present means that under some circumstances that double bond would open up and add certain other material, such as bromine and iodine and react with sulphuric acid. The fact that it is able to take on or react with these other constituents is the reason that we call it unsaturated; whereas, the others which cannot react and take on these additional compounds are considered to be saturated. They have that extra bond filled up with hydrogen already. They are, in other words, saturated compounds which have but little tendency to react further.

The Master: That use of the adjective is in the sense that a substance has reached the maximum of its absorbent power, is that it?

The Witness: Yes, although you would have to limit it to certain reagents. Even the saturated hydrocarbons will react with certain very strong reagents but they do not add on directly such materials as I mentioned.

Q. The normal classification then, probably, is based on this reaction with concentrated sulphuric acid?

A. That is one of the principal classifications of the hydrocarbons.

Q. Now, you have just stated, in answer to questions by the Master, I believe, that the best evidence that the unsaturated hydrocarbons are largely olefines—

A. Yes.

Q. —with two bonds. Now, I want you to state in a little more detail what are the saturated hydrocarbons?

A. In answering that it is desirable to go into a little history. First, practically all the early work which was done on the composition of petroleum has been done on Pennsylvania crude oil, which is what we know as a paraffin base oil, and about the only constituents that could be separated and identified readily were the materials present in the light end of gasoline, say boiling up to 250 degrees Fahrenheit. In that range the number of compounds, instead of being thousands, is down to a matter of probably not over 50. So it is possible by careful work to separate and identify certain of the constituents in that particular cut, and it was found that most all of the saturated constituents belonged to the paraffin series, or single bonded hydrocarbons, either straight or branched chains.

The only other compounds which were identified with certainty in petroleum were the constituents of paraffin wax where recrystalization in addition to other operations it was possible to separate some of the different constituents. Here also the different constituents all belonged to the paraffin series. It is, therefore, not strange that practically all the early workers on petroleum adopted as their working hypothesis the assumption that the saturated compounds in petroleum were the paraffin hydrocarbons and all of the early literature is filled with those two terms, which were used entirely interchangeably. That is, if it is saturated it is paraffin; if it is paraffin it is saturated.

As a matter of fact, however, the more recent work, which has been done in the last ten years particularly has shown that even in Pennsylvania crude oil, when we got to the

products which are heavier than gasoline, and particularly when we get into the lubricating oil range, there is a larger proportion of a different class of saturated hydrocarbons which have a ring-like structure. They are saturated with carbon atoms arranged in the form of rings, or even may have several rings in the same molecule. Such compounds make up the greater proportion of the heavy end of even Pennsylvania crude. For example, if we could remove the paraffin wax and other waxy products amounting to around ten per cent from a Pennsylvania cylinder stock the remaining 90 per cent of lubricating oil appears to belong almost entirely to this napthene series, and other crudes [fol. 793] like mid-continent crude or Gulf Coast crude contain even larger amounts of naphthene hydrocarbons, even down to the light end of the gasoline.

The Master: Your difficulty in making these investigations, is that due to lack of adequate apparatus of not?

The Witness: It is due mainly, to the extreme complexity of the mixture. A chemist has much difficulty in identifying a compound until he can separate a chemical individual that is in a larger number of identical molecules; if they are of the same kind with the same boiling point, he can do a great many things with them, even determine their molecular weight and the number of atoms present. Then he can draw a picture of its structure. The great difficulty is the separating of these many thousands of compounds; if you try to separate them by very close fractions, you may find compounds that boil within one degree of one another, and it is almost impossible to separate them further. If we can get anyone of them separated in pure form we would not have a great deal of difficulty in telling very closely what its composition was, but the difficulty is in getting them separated.

The Master: In other words, you mean if you can work sufficiently closely and accurately with the pressure and temperature you can be certain, of the particular combination of atoms that you will get?

The Witness: Well, we have available certain methods of separation of chemical individuals. One of those is distillation, careful distillation and repeated hundreds of times. One of them is crystallization. One of them is based on chemical reactivity. Now, we can start out with a sample of crude oil and we can, for example, remove the olefines

and leave behind only one or two groups of saturated hydrocarbons. Then we can distill through very fine fractionating columns until we get one material which boils within two degrees Fahrenheit. But if we come to study that we find that is not a single chemical individual, but mave have 150 different compounds in it.

Now, if we fractionate one hundred times more, and we have actualy tried doing something like this type of work, we will get that down until we think we have got only three or four closely related compounds there and we may be able to form a very good estimate of what they are but even this is a very uncertain procedure compared with what it would be if we could actually separate the chemical individuals that were present.

[fol. 794] I may add that in case of identifying paraffin wax the fact that we can crystallize it gives us another means of separation, which is not available for most of the compounds in petroleum, and that is used to identify the constituents of the paraffin wax, and separate them from most of the other constituents of the oil.

Of course, when we get up to the asphaltic hydrocarbons, there we have such extremely high molecular weight materials that even if a chemist isolated and identified any of them it would be pretty hard for us to tell how the atoms are arranged.

We do not know enough about the chemistry of those very high molecular weight compounds.

Now, what proportion of the hydrocarbons in crude oil, using the word in the American sense, as you have done, are olefines?

A. Of the distillable portions of crude oil very little, seldom over five per cent of the constituents are the unsaturated or olefinic hydrocarbons. The residue that are left after distillation, which are mainly asphaltic in character, may be classified as unsaturated hydrocarbons, because they react with sulphuric acid, but as a general rule, the asphalts are considered as a separate and distinct classes of hydrocarbons, that is, highly unsaturated black non-volatile materials. So that, as I said, of the distillable portions, less than five per cent in any crude that I have seen is, properly speaking, unsaturated.

Q. Now, I wish you would just summarize briefly these

statements you have made as to the composition of crude oil?

A. Except for the asphaltic resides, 95 per cent of most crude oils are saturated hydrocarbons, and of the saturated hydrocarbons more than fifty per cent, and probably seventy or eighty per cent, appear to belong to the naphthenic class of hydrocarbons.

Shale oil is oil made by the destructive distillation at very high temperatures of shale rock, or what is sometimes called oil shale; frequently called oil shale.

I have here a sample of oil shale. It is a brown rock-like material which, as you see, contains no oil as such but does contain a brown organic material known as Kerogen. Just as coal or wood or any organic material, if heated to a high temperature, will give a variety of decomposition products, in many cases on all, and leave behind a residue of coke, so [fol. 795] this decomposition makes as one of its products a dark colored, foul smelling, (at least to a respectable petroleum technologist) product known as shale oil. This is almost useless in the form in which it comes off because it has this very bad odor, leaves a large coke residue on distillation or evaporation and is so unstable and unsaturated that generally about all that can be done with is to re-distill it at once to a coke bottom. In this way we leave a considerable mass of coke in the still and get a distillate which is lighter in color and somewhat more stable and more saturated, which is called once run oil.

The Master: That improves it in color and odor?

The Witness: That improves it particularly in color and in the stability and degree of saturation. The original shale oil is very unsaturated; as you re-distill it it becomes somewhat more saturated and the extra carbon, because you can consider all unsaturated carbons as being hydrocarbons which too much carbon for their hydrogen, drops out in the form of coke so that this distillate procured is a much more saturated material than that started with.

Q. Just so the record will show it, you produced a while ago a sample of this shale oil which you have handed to the Master.

A. Which was made from the oil shale of the type which I also show.

The Master: Where did you get this?

The Witness: I got this from the New Jersey Company. I am not sure of its full history.

The Master: You don't know the locality of its origin?

The Witness: No, I don't. I understand it is an American shale.

The Master: Excuse me a minute. Aren't there shales which have a distinctly oil appearance?

The Witness? I have never seen any. It would not be a characteristic oil shale. You have what they sometimes call bituminous sands which are oil sands, which have been exposed so that they can be dug out which do contain oil as such. The test in that respect is to treat them with solvents for petroleum and if you can wash out any considerable amount of oil, it is not then classed as an oil shale. It is classed as bituminous sand or something like that. Oil shales characteristically never have any considerable amount of material which can be extracted by gasoline, for example: It does not exist in the form of oil in the rock. When the rock is heated up, just like coal, (which contains no oil as such), yet we can distill out a considerable amount of liquid material.

The Master: If you were to take it and compress it you would not get a resulting oil?

The Witness: No, that is another type of test that might be applied. Of course, I should say there are many varieties of oil shales, both in this country and abroad, and the character of the oil will vary a great deal between the different varities of shale.

Q. What are the brown coal oils and the lignite oils?

A. These products are made from the destructive distillation of brown coal or of lignite.

The Master: I notice that you are referring distinctly, are you not, now to American products?

The Witness: Yes, though my remarks on shale oil apply to Scottish shales as well as to the American shales.

[fol. 796] In a manner roughly similar to that employed in distilling oil shale and they yield very black, foul smelling more or less worthless types of oils (when considered in comparison with petroleum) which contain, generally, a smaller proportion of nitrogen compounds, and a larger

proportion of oxygen compounds than does shale oil, characteristically.

I have here a sample of the cannel coal which is an American product, and some cannel coal distillate. You will notice that the odor is very different and more pleasing than that of petroleum.

The Master: Brown coal or lignite?

The Witness: That is two designations of a similar product, though there are minor differences. The brown coals have a more brownish appearance but they are all what might be called a low grade coal in the first stages of formation of coal on the way from peat to coal, and which yield larger amounts of liquid prdoucts than the bituminous coals with which we are familiar, but cannel coal is different from brown coal. Brown coal and the brown coal tar distillates are mostly made in Germany from considerable deposits of brown coal which they have there.

Q. Now, I wish you would please compare the constituents of crude petroleum, using the word again in the American sense, with those present in shale oils and similar oils derived from rock-like material.

A. I have not personally done a great deal of work with shale oil. It is well recognized that a very large proportion of shale oil consists of unsaturated and presumably olefinic hydrocarbons. Also there are almost always present from five to twenty per cent of nitrogen compounds which give a characteristic odor to the oil, and as a rule from seventy to ninety per cent of these oils are soluble in concentrated sulphuric acid. Comparing that with petroleum, of course, we say that the seventy——

Q. How about the asphalt in shale oil?

A. There is no asphalt present in shale oil that resembles the asphalt in petroleum. To summarize the comparison we can say that from seventy to ninety per cent of shale oils is made up of compounds which are almost entirely absent from petroleum, namely, the olefines and the nitrogen compounds, where at least ninety-five per cent of petroleum is made of constituents, that is, asphalt and paraffin and naphthene hydrocarbons which are present only to a minor extent in ordinary shale oil. There is a certain amount of paraffin of course in shale oil.

Q. Now, is this contrast between the two products, that is, crude petroleum, and the shale oil borne out by Dr. McKee's book on shale oil?

A. Yes, it is, on page 80 of his book.

The Master: What were the percentages where the two approach a common content?

The Witness: Well, there are probably on the average from ten to thirty per cent of paraffin hydrocarbons in each of the materials, and that is practically the only common kind of compounds in them.

A. (continued.) On this page Dr. McKee discusses at considerable length the large proportion of unsaturated compounds which are present in shale oil, and on page 81 near the bottom he is comparing shale oil with well petroleum. He says that well petroleum "was practically completed saturated, i. e., contained no unsaturated hydrocarbons, and shale petroleum on the other hand contains saturated and unsaturated hydrocarbons, and in addition, [fol. 797] relatively large amounts of oxygen, nitrogen and sulphur compounds, which are not hydrocarbons."

He also states on that page and the next page that the gasoline which is made by cracking shale oil is distinctly less unsaturated than the original shale oil from which it is made.

He states on page 704 of the record that even this less unsaturated material, that is the gasoline, contains over fifty per cent of unsaturated, so that the more unsaturated material,—the shale oil from which it was made,—must contain well over 50 per cent of unsaturated hydrocarbons according to this.

Q. The ordinary compounds, of hydrogen and carbon that you talked about, are all generally classified as organic compounds, are they not?

A. Yes.

Organic compounds in general, which are usually compounds containing carbon, hydrogen and oxygen, and occasionally sulphur and nitrogen or other materials, are invariably decomposed, if heated to a high enough temperature. When decomposed by very high temperatures, the products are generally coke, and a large amount of gas, including water vapor. At lower temperatures a wide variety

of different compounds can be produced, depending on what you start with and on the temperature to which you heat.

Petroleum, of course, is no exception to this rule. If you want to make a large yield of any particular compounds it is necessary to very carefully control the temperature, pressure and the time and other conditions in order to get it; and if you work under some other set of conditions you get quite a different type of decomposition. If you heated to very high temperatures the products are mainly coke and gas, just as for other organic compounds.

The Master: Such as wood and coal?

The Witness: Yes. Cracking is a very broad term which covers almost any splitting apart or joining together of hydrocarbon molecules. The reactions are extremely complex. There is not very much generalization that one can make in regard to them. That is, things that are true for [fol. 798] one set of conditions or for one type of oil are not true for some other type or set of conditions. About all we can say about the reactions in the cracking of oil is that there are generally produced by cracking both lighter constituents and heavier constituents; although we usually focus our attention mainly on the lighter constituents. Also if cracking is carried to an extent sufficient to make any considerable amount of gasoline, we will always make coke and gas.

The Master: Cracking generally means simply the disruption of the molecular substance?

The Witness: Disruption or the joining together. Both take place simultaneously and both are part of the same operation.

The Master: One resulting from the other?

The Witness: Possibly; we cannot say for sure.

Q. On pages 396 and 397 of the record the plaintiff's expert gave graphic formulae for some reactions which he asserted occurred in cracking. What is your opinion as to the occurrence of these reactions?

A. Plaintiffs' expert sketches on page 396 a hypothetical chemical reaction, the first one starting with a saturated paraffin hydrocarbon, and forming one molecule of saturated hydrocarbon boiling in the gasoline range, and an unsaturated hydrocarbon boiling in the gasoline range. On the next page he takes this unsaturated hydrocarbon and sketches another hypothetical reaction which results in

the formation of a saturated, gaseous hydrocarbon, a highly unsaturated material, with two double bonds, which we call a diolefine, which boils in the gasoline range, and a molecule of coke and a molecule of hydrogen. Now, these types of reaction are of course theoretically possible and are just the type of reactions that a chemist would likely draw up if asked what probably happens. As a matter of fact, however, they are far from representing anywhere near what actually takes place in the cracking reaction. I wouldn't say that possibly one of the molecules in that complex mixture might not split up in this very way, but these reactions result in the formation of nearly fifty per cent of unsatu*ar*ted hydrocarbons, over ten per cent of coke and over fifteen per cent of gas, and if these reactions were actually representative of what takes place it would be a very unsatisfactory, non-commercial process for producing any useful result.

Actually, in a process of the Burton type at moderately [fol. 799] low temperature, with good charging stock, the reactions as we know are quite different from that in most respects. Unfortunately we do not know just what the reactions are, but we do know that in the first place of the total product formed, including both the gasoline distillate and the pressure tar left behind, only about twenty per cent consists of unsaturated hydrocarbons soluble in sulphuric acid. And, in addition to making the lighter products, which are illustrated by the chemical reactions of plaintiffs' expert, we make something like 40 per cent of heavier hydrocarbons, due to some chemical change which takes place in the course of cracking, these heavier hydrocarbons being left behind in the still in the pressure tar which is withdrawn at the end of the operation. As a matter of fact, it is only because the course of the cracking reactions which actually take place are of this peculiar and unpredictable type and quite different from the hypothetical reactions of plaintiff's expert which makes that it is a commercial, practical process, giving, as I indicated, very little gas and coke and a rather highly saturated gasoline.

The Master: Do I understand that when you have applied means to disrupt the molecular structure of your substance you do not know how and why the atoms unite?

The Witness: In cracking, which is largely an empirical proposition, you must study your stocks and your conditions and find out what happens and then rely on the fact that if you do exactly the same thing again the same results will be obtained. That is what the cracking art is really based on.

Q. You do know it forms very much more of the heavier compounds than what you started with.

A. Yes.

Mr. Wiles: I may say, your Honor, that these heavier compounds will have to be considered in considerable detail when we get to that twine oil patent, because that is on some of these heavy compounds that are formed.

The Master: Yes.

A. Hydrocarbons vary greatly in the temperatures to which they must be heated in order to decompose or crack them. Some hydrocarbons, like benzol and methane are comparatively stable even at temperatures as high as 900 or 1,000 degrees Fahrenheit, while other hydrocarbons, such as the terpenes in terpentine, will decompose quite readily at temperatures around 300 to 400 Fahrenheit; in fact, some of the less common hydrocarbons will crack or [fol. 800] decompose at very little above room temperature, so that there is very little generalization, you can make as to cracking temperatures for hydrocarbons. It covers the whole range from 100 degrees to 900 or 1,000, depending upon the hydrocarbon.

It so happens that for the two kinds of hydrocarbons which form the main portion of petroleum oils, that is the paraffins and the napthenes, the temperature at which cracking begins is fairly similar. If we speak of when appreciable cracking begins, it depends somewhat upon what we are looking for. If we want to make gasoline, for example, we may consider that one or two per cent is in appreciable but in some operations like the distilling of lubricating oils, particularly when we are distilling a large batch in vacuum, even a very little cracking is bothersome, because it spoils the viscosity of the lubricating oils and forms some gas and some light distillates which will spoil the vacuum.

Using that definition of the word "appreciable", I should say that appreciable cracking on most petroleum oils begins between 600 and 650 degrees Fahrenheit.

I have never seen any evidence of any petroleum, in the American sense, cracking at temperatures below 800 Fahrenheit. Apparently if any of these other less stable hydrocarbons and types of hydrocarbons which I have spoken of were present in petroleum at any time, they were changed by the age-long period of storage which they have undergone at high temperatures in the interior of the earth. Of course that prolonged stabilizing action practically eliminates all the less stable hydrocarbons.

In properly designed pipe stills, on account of the short time during which the oil is passing through the pipe still, as compared with the relatively long time that oil is held at a cracking temperature in a big batch still, you can go to considerably higher temperatures before you begin to get appreciable cracking. Almost any pipe still will show no cracking up to 700 degrees Fahrenheit and the faster the oil is put through the tubes the higher temperature you can go to before you begin to see appreciable signs of cracking. That does not mean it would not crack if held for a longer time at the higher temperature, but it does not begin to crack in the short time it is held at or near the maximum or outlet temperature.

[fol. 801] Q. Now, tell us at what temperature does the rate of cracking of hydrocarbons in petroleum become high enough to be commercial?

A. At around 720 degrees. I do not think there is any commercial cracking process, that considers it is cracking at a rate fast enough to be commercial at temperature below 720 degrees; although you could make quite a bit of gasoline in a period of a week even at as low a temperature as 700.

Q. Now, do you dispute the statement of plaintiff's expert on page 671 of the record that shale oils begin to crack at temperatures around 350?

A. No. I have not done much work with shale oils and I cannot say from my own experience just where they do begin to crack, but it is known that they contain many much less stable hydrocarbons that petroleum, which is not surprising in view of the fact that they have not been subject-d to this age-long period of storage. They have been

rather freshly made by the decomposition of the shale rock and they are known to be very much less stable.

The cracking reactions in cracking shale oil are really quite different from those that take place in cracking petroleum. That is another reason why we might expect that the temperature at which the reactions would begin would be quite different. They are an entirely different type of chemical reactions for the most part. I think Dr. McKee's book confirms this statement as to the radically different nature of the reaction involved in cracking shale oil, when he says on page 81:

"When a highly unsaturated shale oil is cracked the results obtained are apparently just the reverse of those obtained with petroleum."

I regard the great difference in temperature at which this cracking begins as just another evidence of the marked difference in the composition of shale oil and petroleum, and the great difference in the nature of the cracking of the two materials.

Q. Now, plaintiff's expert concludes from his shale oil experiments that petroleum oils begin to crack at the same temperatures. Is his reasoning sound and is his conclusion correct?

A. His reasoning is certainly not sound in view of the great difference in the chemical composition of the two [fol. 802] materials. The best evidence that his conclusion is not correct, is the difference between 350 degrees, at which he indicates shale oil begins to crack, and 625 degrees at which petroleum oils actually begin to crack.

Q. Now, plaintiff's expert also stated on the next page, 672, of the record, that commercial cracking processes operate in the range between 600 Fahrenheit and 700 Fahrenheit; is that true as a matter of fact?

A. No, it is not. There is no process, so far as I know, which either does or can operate in that temperature range on petroleum.

The Master: You used that in the sense of final temperature?

The Witness: Well, either final temperature or the temperature at which the body of oil is held. We have in cracking petroleum, as in ordinary distillation, two different

types of processes, the tube cracking processes, where we talk about the maximum temperatures and the bulk distillation process, where we held a large body of oil at the same temperature.

The Master: In your commercial cracking you run either the still or your bodies of oil through the still at a much higher temperature?

The Witness: Yes.

The Master: And you say it begins to crack at 625?

The Witness: Yes, but not at any commercial rate.

The Master: That is, you hold your temperature higher.

The Witness: Yes.

Q. And there is no process running that does hold six to seven hundred before you give temperatures a running temperature?

A. No. Commercial cracking processes (if you include the cracking to make gas in the gas industry) operate from a minimum of around 720 to a maximum of around 1500 or higher. Temperatures in the gas-making art are somewhat uncertain. I should make clear that the processes involved here,—defendants' processes,—all operate in the rather narrow range between 720 and about 900 degrees Fahrenheit.

The Master: As the final temperature?

The Witness: As the final temperature. Just to clear up that point, that does refer to final temperature but it [fol. 803] also refers to the minimum temperature at which any body of oil is ever held in order to bring about cracking. The lower temperatures are merely temperatures that are passed through necessarily on the way up to the commercial cracking temperatures.

Q. In the commercial distillation of petroleum oils is there ever any deposition of coke when the body of oil is below 600?

A. Yes, there is. Whenever you try to force any considerable amount of heat through a tube or still wall into a body or stream of oil, the temperature of the still wall gets considerably hotter than the main body of the oil inside, and in immediate contact with that hot surface there is necessarily a film of oil which is superheated to the surface temperature. Now, if you get rapid circulation of the

oil in the still it is possible to keep that film very thin and not very much superheated, but if the rate of circulation is not rapid or if there is any deposition of dirt or salt or sediment to help impede that circulation it can be very considerably superheated. It might be thought that in a large shell still the boiling which rapidly goes on would keep that film agitated, but as a matter of fact, inside of such a still the pressure on the oil at the bottom of the still, where the oil is heated, is so much greater than in the oil on top, that it only boils when it gets near the top, and you don't get much agitating action near the bottom; for that reason, if you want to get rapid circulation near the bottom it is usually necessary to introduce steam near the bottom. If you do not have rapid circulation to prevent local overheating of the film of oil, and even if you did have good circulation and the body of oil gets quite close to a cracking temperature, it is quite easy to get local decomposition in a very thin film of oil on the heating surfaces. In the course of weeks of such operations you may have a thin layer of coke which may eventually have to be removed in order to keep the still bottom from getting overheated, without any perceptible change having taken place in the body of the oil, and without any other indication that cracking is taking place. In other words, the cracking is not taking place in the body of the oil but it has taken place in local superheated film which has gotten in spots maybe as high as 750 degrees.

As I said, running crude oil, or any stock that contains dirt or salt or anything that will settle out on the still bottom tends to increase the localized cracking because it tends [fol. 804] to make a very much thicker stationary film which is likely to be kept at a higher temperature.

Q. This little layer of coke doesn't make the process, in any proper sense, a cracking process?

A. No, it would not be termed a cracking process in any proper sense.

Vapor pressure is the general property of liquids, which may be stated to represent their tendency to form vapor. This tendency is a definite physical property of a given compound at a given temperature. That is, any one liquid will always have the same vapor pressure at the same temperature. As you increase the temperature, however, the tendency to vaporize increases very rapidly. Thus, water

at 70 degrees Fahrenheit has a vapor pressure of about one third pound per square inch. At 150 degrees Fahrenheit it has a vapor pressure of nearly 4 pounds, and by the time you get to two hundred and twelve Fahrenheit, its vapor pressure is 14.7 pounds. When it reaches that temperature its vapor pressure is equal to the pressure of the atmosphere at sea level, and then what we call boiling takes place. At 250 degrees F. the vapor pressure of water is 30 pounds or more than two atmospheres.

This vapor pressure curve is a gradually increasing curve all the way up.

We are very familiar, of course, with this vapor pressure in the steam tables, because of the wide use of steam in industry.

In other words, vapor pressure is just another way of specifying the pressure at which a liquid begins to boil of itself, if nothing else is present.

The same thing is true of vapor pressures below 14.7 pounds. Not only is a vapor pressure curve available for water in the common steam tables, but within recent years rather careful vapor pressure curves have been made for a great many hydrocarbons. Of course these curves are not the same as for water, but we can pretty well tell, if we know the boiling point of a hydrocarbon at atmospheric pressure, what its boiling point will be at some other pressure—or what its vapor pressure will be at some other temperature, because we have a general family of curves, and most hydrocarbons belong somewhere in that family.

The Master: In other words you plotted a table?

The Witness: We plotted a series of curves. Dr. Bahlke and I, about three years ago, compiled all the available data and calculated some additional data on vapor pressure curves, and other physical properties of the paraffine hydrocarbons, and we have here a reprint of that article, which I produce and call your attention particularly to figure one, on page 116, which shows the family of curves for the paraffine hydrocarbons. This is the temperature scale, and this is the pressure scale, so that this shows how the vapor pressure goes up as the temperature goes up for all the different hydrocarbons.

Mr. Wiles: I offer in evidence this reprint as Defendants' "Exhibit No. 398," and we may use it for reference to other physical properties, as well.

[fol. 805] Mr. Rauber: Is this all based on the witnesses' own work?

The Witness: Not altogether on my experimental work. It is a compilation—it is the result of years and years of experiment, based on certain principles recognized as being sound.

The said reprint, so offered, was admitted and read in evidence, was marked "Exhibit No. 398", and the same is herewith returned.

Q. And you use it in practice and find it reliable for determining the physical constants in question?

A. It is practically our Bible that we use in designing equipment. Of course it was prepared and published long before this case.

Mr. Rauber: About what proportion of the work in this, is your own original work?

The Witness: A very small proportion. I think it shows in the articles that very little of the data is my own original data. Some of this specific heat data is mine, in conjunction with another man.

Mr. Rauber: But you don't know how accurate all these figures are, do you?

The Witness: No, I simply considered this to be the best available data in the literature. We have compiled it critically.

Q. These are, in fact, the ones that your Company uses in the designing of equipment and relies on in its regular commercial work, are they not?

A. Yes, sir, and I happen to know that a number of other companies employ them in the same way.

Q. Now, do these principles of vapor pressure which you have just outlined, apply to mixtures of more than one hydrocarbon?

A. As a general rule the vapor pressure of the mixture is approximately, though not exactly a properly weighted average of the vapor pressures of the individual hydrocarbons present in the mixture.

It is also true that if we have in a bottle some liquid and some vapor, theoretically all of the compounds present in the liquid, are also present, to a lesser extent, in the vapor above it.

Of course if we have a mixture of, say, gasoline and kerosene in a bottle, the proportion of gasoline molecules in the vapor, will be greater than the kerosene molecules, because gasoline has a greater vapor pressure at a given temperature, and has a greater tendency to escape. That is why it boils at a lower temperature, and the fact that the gasoline has a greater vapor pressure is the thing that makes it of more practical use in the gasoline engine.

Q. Now, do the general laws that you have described as to the change in vapor pressure, apply, also, in the case of mixtures?

A. They apply approximately, though not precisely. It is the best guess we can make, as a rule.

Q. Now, there are a number of points where, both in defendants' patent and in the prior art, it is necessary, as I see it, to know ordinary refining practice, in order to understand what the patents are talking about, and so I am going to ask you to describe, briefly, some of the principal operations that are carried out, and the products made in the ordinary refinery, and you can illustrate those with samples. In the first place, describe what the principal commercial methods of distilling crude oil, are.

A. The principal commercial methods of distilling crude oil are commonly known as skimming, or running for steam [fol. 806] refined and cooking. They are all carried on at atmospheric pressure, substantially.

Q. Do these methods always yield, in the first instance, well separated cuts of the desired boiling range?

A. No, they do not. In many cases the first cuts that are taken off boil over a very wide range, and in order to make the desired secondary cuts, or finished products, from them, they must be redistilled.

The Master: The first cuts at the bottom of the tower, or——

The Witness: No, I am thinking of taking a large body of oil in a still and heating it gradually. First the gasoline begins to come off, and we keep putting that in one tank until the stream reaches a certain gravity, and then successively to other tanks for such periods as will give portions of distillate suitable to be re-run for the desired finished products.

Those are what are called our primary cuts, in which the oil is first divided, when it comes from the still and they

are re-distilled to make the secondary cuts, the gasoline and kerosene.

For instance, this first cut taken off may boil over a wide range, and it is re-run and split up again.

The primary cuts vary greatly in different refineries—some will take off very wide cuts, that do not bear very much resemblance to the finished products, and others will take off narrower cuts, which are practically finished.

So that in considering the ordinary refinery cuts, we can generalize a great deal better when we talk about secondary cuts after whatever re-running may be necessary in certain refineries, because those secondary cuts are rather well standardized.

The Master: You are referring to the low temperature distillation, or——

The Witness: These are ordinary products of running crude oil at atmospheric pressure.

Q. In your discussion, for simplification, I wish you would refer to those secondary cuts, because, as I think you have stated, it will considerably simplify things if you will for example, consider gasoline and kerosene as separate cuts, because they are ultimately separated. With that understanding please describe the ordinary skimming method of distillation, as carried out in batch stills, that be- [fol. 807] ing the first of the three that you mentioned as the principal methods of running crude.

A. In the skimming operation, as ordinarily carried out in a batch still, the oil is charged into a still frequently fourteen feet in diameter by forty feet long, which holds a thousand barrels of crude oil. It is then gradually heated up, and first the gasoline and then sometimes the kerosene is taken off and the still is then cooled down and the tar withdrawn and used as fuel.

If kerosene is taken off steam is usually introduced to avoid any danger of cracking, although only a little localized cracking is likely to take place until the kerosene is off.

If only gasoline is taken off, steam is sometimes used; and sometimes not.

This results in making three products—gasoline, kerosene—if that be made—and what we call reduced crude oil.

I have, also, samples of these products made from mid-continent crude. They were made by the skimming process. Here is the reduced crude, which is the material which results after taking off around forty-five per cent. of the crude oil—in this particular case.

Q. Now, on what type of oil is this skimming operation normally practiced?

A. It is best suited for handling very heavy oils, without much lubricating value, and those heavy crude oils which contain large amounts of asphalt. About all you can do with the products heavier than kerosene from such crudes, is to sell them as fuel oil.

It is also practiced, to a considerable extent, on crudes like mid-continent crude, by smaller refineries, which do not have the equipment for making lubricating oils. Skimming operations are used on crudes with the higher asphalt contents, such as California crudes.

Running for steam refined oil is a method quite generally employed on Pennsylvania paraffine base oils, which contain large quantities of very valuable heavy lubricating oils.

In that case the operation starts off very much like the skimming operation, but instead of stopping when the kerosene is off, it is continued, taking off a cut of gas oil, and a cut of paraffin distillate, which is also known as wax distillate or pressing distillate. The residue left in the still [fol. 808] is then a heavy, rather dark colored material, known as steam refined stock.

I forgot to say that toward the last stages of this operation large amounts of steam are used, in order to distill out the lubricants, without any danger or cracking taking place, the still temperature being held below 625 degrees, as a rule.

The cylinder stock thus produced, is also rather waxy, because it contains a considerable amount of petroleum.

In finishing up those oils, they are diluted with two or three times their volume of nap-tha, and chilled to a low temperature to separate out the petrolatum, which is removed either by centrifugal separation, or by setting for several days in large cold settlers frequently about twenty feet in diameter and 40 feet high.

Then this oil is run through Fuller's earth, in granular form, to remove the coloring matter and other impurities,

and it is then taken to stills where the nap-tha is distilled off, and finished cylinder oil is left behind. This is a very heavy undistilled lubricant, which is used for steam cylinders, and as a compounding material in making up a great many heavy oils. I have here samples, also, of this finished oil.

This method is also used on mid-continent crude, but with this difference, that on account of mid-continent oils containing considerable amounts of asphalt, the operation of reducing is interrupted—excuse me—I will explain, here, that I will probably inadvertently use the term reducing, which is a common refining term for distilling off light ends from any product.

The reducing is interrupted after the kerosene is off, when we have a product very much like the reduced crude, a sample of which I have offered, and it is then treated with acid, to remove most of the asphalt present, before the distillation is completed, taking off the gas oil and the paraffine distillate.

Then the finally reduced crude—the second reduced crude is treated very much like the Pennsylvania products are, making the finished lubricating oils for cylinder stock.

Here (witness indicating a sample) is the heavy product of the second reduction, after the paraffine distillate has been distilled off, and you are left with this (pointing) which is practically solid because of its wax content. This [fol. 809] (pointing) is the refined cylinder oil made from that, by settling and filtration.

Q. Now, just to summarize in relation to these samples—you start with this mid-continent crude, of which you have produced a sample, and you would first take off the gasoline and then the kerosene, and then you would stop and withdraw the oil from the still, and what you took out would be this reduced crude from the skimming?

A. Essentially so.

Q. And then you take that sample and treat it with acid?

A. Yes, sir.

Q. And the asphalt is precipitated out?

A. Yes, sir.

Q. And then you put it back in the still and reduce it some more, and you get this thick product, that is nearly solid, and which is cylinder stock?

A. Yes, sir.

Q. Then that material is diluted with acid?

A. Diluted with naphtha.

Q. Chilled to a very low temperature, to solidify the wax, and allowed to settle, and the wax drops out?

A. Yes, sir.

Q. And then you filter the oil that was left at the top, and have this finished cylinder oil, which is a lubricant.

A. We filter, and then we distill off the naphtha, and then we have the finished cylinder oil.

Q. Now, have you a sample, also, of the petrolatum that you get out of this settling operation, when you settle the diluted cylinder stock?

A. Yes. This is, of course, a very highly refined sample of petrolatum, that is made from the material that settles out of the cylinder stock.

The third and coking method of operation is one that is very generally carried out on mid-continent types of crude oil, since the demand for cylinder stock is by no means sufficient to justify running all the oil to make cylinder stock. Furthermore, a great many refineries lack the equipment necessary for that. So that the bulk of this oil is distilled down to a coke bottom in batch stills, of the same general size as those used in the skimming operation—that is, fourteen by forty. In this operation the cuts taken off are sim- [fol. 810] ilar to those mentioned in the other operations, except for the fact that cracking takes place in the latter part of the distillation. In this case we are not using steam. We are encouraging cracking rather than discouraging it. We first take off gasoline and kerosene, and then gas oil, and then, about the time the gas oil begins to come off, there is a small amount of cracking taking place, and from then on a considerable amount of cracking takes places.

The still generally takes off from three to four per cent. of the crude oil per hour. That has been somewhat speeded up in *the* recent years, but that is the general order in most coking operations.

The next cut which comes off is the paraffine distillate, and with it comes a certain amount of cracked material, and, therefore, most coking stills are equipped with towers, which serve as fractional condensers, and roughly separate the heavy oils, which are distilled from the crude and lighter products that have been made by cracking, so that

the paraffin distillate goes out one line, and what is called light paraffin gas oil goes out another, most of this gas oil being the product of cracking.

The great bulk of it boils in the gas oil and kerosene range, although a little gasoline is formed—generally only a matter of a couple of per cent. of the crude.

After the paraffine distillate comes off, the next cut is the slop cut, which contains some heavy lubricating oils and what we call unpressable, or slop wax, which cannot be handled in the ordinary method of treating paraffine distillates.

The last cut that comes off is known as still wax, which is a very heavy, sticky product, which is very slightly volatile, and comes off from the last stages of the coking operation, when the still temperature becomes quite high.

Left in the still, at the end of this operation, is about four per cent. of coke, which forms a layer of about a foot thick. In the last hours of the coking operation, still bottoms are red hot, and the coke tends to bulge the still, by its own weight, but there is comparatively little danger even if the bottom should split and let some of the very heavy oil down into the fire box.

I have some samples of the products.

The Master: Thus far you have been discussing batch stills?

[fol. 811] The Witness: Yes, sir. That coke, of course, is dug out as soon as it is cool enough for the men to go in, and is used as fuel.

The slop cut is a product which we cannot handle at all satisfactorily, and so it is either put back into the crude oil, or re-run separately, making other products from it.

Q. It eventually ends up in one of these other cuts?

A. Yes, sir, including coke and gas.

Q. So that it disappears from the story.

A. I have here samples of the heavier cuts from this coking operation. I haven't separate samples for the gasoline and kerosene, because they are very similar to those already produced.

Q. For the sake of the record, in producing these samples, let us go through it this way——

A. I thought I would put them all in at the end, Mr. Wiles.

Q. I don't mean to offer them, but you start on this operation with a still full of this mid-continent crude oil, such as you have produced.

A. Yes, and you take off, first, the gasoline, second the kerosene, third the gas oil, fourth the paraffine distillate, or wax distillate, fifth, the slop cut, consisting of unpressable wax and heavy oil, and sixth, the still wax, which is almost solid, and a very sticky, product, and we have left in the still coke in layers of very considerable thickness.

Q. Now, what becomes of the heavy lubricants and the asphalt, when you practice this coking method?

A. The heavy lubricants, such as those in the cylinder oil and the asphalt, are, for the most part, cracked, making lighter lubricants, gas oil and kerosene, and a comparatively small amount—not over two or three per cent.—of gasoline. Also, towards the end of the coking operation, there is a considerable quantity of gas given off by the coking stills, as the product of cracking.

When they were running for maximum yield of kerosene, and didn't care so much for lubricants, it was the effort to run somewhat slower than at present, to produce as much kerosene as possible by the cracking operation.

[fol. 812] Mr. Rauber: What time is that you are speaking of?

The Witness: From the period in the beginning of the industry to 1910.

Mr. Rauber: You are not speaking of your own knowledge, now, are you?

The Witness: No, I am not speaking of my own personal knowledge.

The Master: That is historical.

Q. And when the coking operation is run on a slower schedule, such as you have mentioned, what becomes of the paraffine distillate?

A. Considerable quantities of even the lighter lubricants and paraffine distillates are cracked in this method of operation. In other words, on the fast scale, only the very heavy lubricants and the asphalt are cracked, but on the slower schedule you will have the lighter lubricants in the still for a longer time and get more kerosene and less lubricant.

Q. Now, you have described, or mentioned, and produced a sample of the paraffine distillate, and you have stated that you get that cut both in the coking operation and when you are running for steam refined. Now, tell us what is done with that, that you get in either of those operations?

A. The paraffine distillate cut contains considerable amount of suspended wax, amounting to, generally five or ten per cent, in the case of mid-continent oils, and it is chilled down to a very low temperature and forced at fairly high pressure through filter presses, which strain out the wax in the form of a cake.

In order for the oil to flow freely through those filter presses, it is, of course, necessary that it have considerable amount of light ends, because if we have nothing but heavy lubricants there, and at low temperature we would not be able to pump it through. The oil, freed from the wax, is then taken to what is known as reducing stills, where, with the aid of steam the light paraffine ends are taken off.

Then the next products which come off as the light lubricants generally known as the pale oils, and as a residue in the still we leave oils which are generally known as red oils, somewhat heavier in viscosity. Both those oils are generally finished up by treating them with acid or Fuller's earth, or other means, to make the oils lighter in color. The paraffin wax, which is left on the presses as cakes, naturally contains quite a good deal of oil, and is refined by various operations, to make finished white paraffin wax. I have samples of these.

[fol. 813] Q. Before you produce those, you spoke of having a diluent in the pressing distillate. That diluent is not added, but it is the gas oil that is left in there—is that true?

A. It is simply left in, in the operation of running and making the stills. We don't put in gas oil, but we let the gas oil go over here (indicating pressing distillate) and then it is taken out later. That is different from the way we handle the cylinder stock, where we take out everything and then thin it out with naphtha. But the result is the same.

Q. Now, will you produce your samples?

A. This shows samples of finished paraffine wax, finished pale oil, finished red oil, and, of course, there is something corresponding to this gas oil, (indicating) which is first taken off from this (indicating) paraffin distillate.

Of course the marked improvement in color is mainly through the use of acid, or filtering through Fuller's earth, and all the finished products have a better color than the original oil.

Q. These products—the three lubricants—the red oil, pale oil and the cylinder stock—are ready for the market—they are finished and thoroughly refined.

A. Yes, sir.

The Master: There are only two here—pale oil and red oil. The red oil is a heavy lubricant?

The Witness: Yes, sir—although nowhere near as heavy as this, (indicating cylinder oil). You see this is a distilled product.

The Master: As distinguished from the cylinder oil?

The Witness: Yes, sir. These three general types of oil are the main sources of most of the lubricants, running in different proportions. Of course we take off different pale oils and different red oils.

The Master: When you ask for either light or medium or heavy oils, you get something between this red oil and this cylinder oil?

The Witness: The light motor oil has a viscosity between the pale oil and the red oil that you have here. A medium oil would be very nearly the same as the red oil. Of course it will differ for different companies. There is no uniform name. Cylinder stock by itself, is not used in automobile lubricants. Sometimes cylinder oil is added to the red oil to make the heavy automobile lubricants.

Q. Now, are the methods of distillation which you have described always carried out as batch operations?

[fol. 814] A. No. Both the skimming and the running for steam refined, are, to an increasing extent, being run by continuous methods of operation.

These continuous methods take two forms—particularly in running for steam refined, and sometimes in skimming. The operation is carried out in a series of stills. To make it as simple as possible, we might say we have a series of four stills, and the oil, after going through some heat exchanges, passes through the first thousand barrel still, and is distilled and the gasoline taken out, and the oil then passes into the next, where the kerosene will be taken off, and into the next, where the gas oil is taken off, and, later,

the pressing distillate is taken off from the fourth still, and the streams from each still being led off to proper receivers and the tar drawn continuously from the bottom of the last still.

Another method of operation is to run in what are known as pipe stills, where the oil is pumped through a pipe that is exposed to furnace gases and is gradually heated up to a certain maximum temperature, which is sufficient to vaporize the desired portion of the oil, and then it is conducted into a separating chamber, where the vapors pass off from the top, and the tar is withdrawn from the bottom, and these vapors are sometimes split up into two or more cuts.

As to the third, or cooking method of operation, there have been a good many attempts to put it on a continuous basis, but so far as I know none of them have ever been successful. I have tried out one or two, myself, but they have not been successful.

Q. Do you say the three methods of running crude you have described, were commonly and generally used before 1912?

A. Yes, they were.

Q. What is the practical mechanism of separating these various products which you have described?

A. As I indicated, the stillman goes by the viscosity, or the gravity, or some such property of the stream, which can be used to determine when the streams should be switched. Those cut points, of course, are set by the management, in such a way that the average composition of material in the cut tank will approximate the fundamental requirement of the products that it is desired to make, and it is mainly the fundamental requirement of the products, that determines just where the cut points are to be made.

The Master: Do you say that in the bubble tower the separation is automatic?

[fol. 815] The Witness: Not entirely. You have to determine the temperature at which you operate your bubble tower, because that is responsible for the composition of all the cuts, even if you run a batch still with a bubble tower, as your product comes over it will gradually decrease in gravity and you have to make cuts.

On a continuous still if you can hold the temperature of your bubble tower at the right point, and have a properly

designed tower, you will always get off material of a certain composition, as long as your charging stock doesn't change.

There are many in the industry who think that you can get a series of cuts from a bubble tower. As a matter of fact, there is only one closely fractionated cut at the top of the bubble tower. The intermediate cuts will vary in gravity, but they are not closely fractionated cuts. The bubble tower only finishes its work at the top of the tower, in the simple types of bubble towers. You can make a very complicated arrangement which will accomplish a very different purpose, but the ordinary bubble tower only has that one cut.

Q. Now, you used the expression "stream"—the stillman tests the stream—by that you meant the distillate that is pouring out of the condensers, from which he takes samples from time to time?

A. Yes, the stream, after being condensed, flows from the condenser into what is called the look box, where the stillman can inspect it and take samples and see what the properties of the stream are at that moment, and by that he governs his orders to the fireman, or his changes in the setting of valves and the turning of the stream from one tank to another.

Q. The term, "cuts" is used both as a noun and a verb, in the industry. As a verb it means simply switching the stream from one receiver to another. And the word "cuts" used as a noun, is the entire result that goes in to a tank.

Q. Now, what are the fundamental considerations which determine the first cut point—that is, between gasoline and kerosene.

A. Since gasoline is the more valuable product, more valuable than kerosene, the natural tendency is to put as [fol. 816] much material in the gasoline as can go there without injuring its properties, and keeping it from being an entirely satisfactory and marketable motor fuel. The exact location of that cut point is controlled by quality requirements, and, of course, is going to be dependent on the law of supply and demand and competitive conditions, and will vary up and down, depending upon the supply of crude, and other factors.

Q. Does the automobile engine design have an effect on it, too?

A. Yes, it would have some effect on what would be a satisfactory gasoline, particularly if all cars adopted certain improvements in design.

The fundamental reasons why we must limit the amount of heavy material which can go into gasoline, are three:

First, if we put in too much heavy ends, we would have a lot of unvaporized gasoline in the manifold. The carburetor mixes the gasoline with a stream of air, and we count on vaporizing most of it before it reaches the cylinders, and the ordinary manifold will not distribute the liquid gasoline to the different cylinders. If it is all vaporized of course the distributor is the same for all the cylinders, but if you have a large proportion of unvaporized gasoline, some of that will be swept around into certain cylinders, resulting in unsatisfactory operation. So the first reason for specifying not to many heavy ends is that it will be sufficiently vaporized in the carburetor and in the manifold.

The second requirement is based on the fact that if too much heavy ends are thrown into the cylinder, they will dissolve in the oil on the cylinder walls and wash it down into the crank case, and in time thin out the lubricating oil in the crank case, causing what we call dilution of the oil in the crank case. The larger the proportion of the heavy ends the more this undesirable action takes place.

The third factor which tends to limit the amount of heavy ends that can be put in, is the fact that the heavy constituents of gasoline tend to knock, if the engine is carboned up, and in the case of some gasoline they knock very badly. This limits the amount of heavy material that it is desirable to put in. Those are the three factors that tend to limit the amount that can be put into gasoline, and in cooperation with the laws of supply and demand, determine the specifications on which gasoline is made at any particular time.

As a general rule, the gasoline marketed by most of the larger companies, will have at least ninety per cent. of the gasoline coming over between 380 and 390° F., and all of it boiling over below a final end point of around 430.

These standards which have been adopted, are a little better than the Government specifications of ninety per cent. off at 392 and all over at 437 Fahrenheit.

Of course it is perfectly possible to buy gasolines which do not meet the Government specifications.

There is one statement I would like to make regarding the article on the physical properties of the paraffin hydrocarbons. With regard to one particular group of data, that on the specific heats of oils, which I do not expect to use in this case, it occurs to me, however, that we now know of better data than are given in this article. I pointed out in the article that the data on the specific heats of oils were very meagre and that some more work should be done. That work has since been done in my laboratory under my direction and has been published by a paper written by Fortsch & Whitman in a recent number of Industrial and Engineering Chemistry and that is the better data on specific heat.

Q. Now, what factors determined the lower cut point or end point on kerosene?

A. Fundamentally, we must not leave enough light ends [fol. 817] in the kerosene to make the product dangerous to handle. The specific criterion that is generally used to measure danger in handling is what is known as the flash point, the flash point being the temperature to which kerosene must be heated before it gives off a high enough concentration of vapors to ignite. The fire point is also sometimes used in this same connection and it is a temperature, generally about 20 degrees higher, to which the kerosene must be heated before it will ignite and continue to burn, as distinguished from merely flashing and going out again.

Legal requirements in many states and the requirements of practically all manufacturers insist that the flash point of kerosene shall not be below 110° F., and frequently not below 125. If you take a given light distillate containing both kerosene and gasoline and distill off the light ends until you have left a product which flashes above 125, in general there is no material which has a boiling point below 300 degress Fahrenheit in the kerosene, or at least the appreciable amount of such material, so that that might be stated, in a way, to constitute the absolute lower limit for the light ends of kerosene, and hence aid in fixing the cut point.

It is to be noted that the kerosenes give off enough vapors to give a flash at a temperature far below its boiling point (that is that same phenomenon that I have spoken of, in

discussing vapor pressure); the vapor pressure in kerosene necessary to produce a flash point is only about 15 pounds, whereas, in order to boil, the kerosene must exert a pressure of 14.7 pounds per square inch. That means about one per cent by weight of kerosene vapors in air. That is all that is required to form an explosive mixture, whereas it requires 100 per cent in the air before boiling will really begin. That is by weight. As a matter of fact, however, this flash point limitation is more of a theoretical than a practical one today, and as I pointed out, there is material boiling above 300, which can go into kerosene, which can also be used as part of the heavy ends of the gasoline. As long as the gasoline is more valuable product, generally we put as much as possible of these light ends into gasoline, and leave only what cannot be commercially used in the gasoline to go into the kerosene. That does not mean, however, that there is nothing boiling below 430 Fahrenheit (which is, as I have said, the ordinary end point of gasoline), in the kerosene cut, because in practically any [fol. 818] crude oil or cracked distillate there is much more material boiling between around 370 Fahrenheit and 430 Fahrenheit than can possibly be used in gasoline, and it is therefore put in kerosene and it is very valuable there because it makes it easier to light the flame of a lamp, and because the presence of that amount of light ends tends to reduce the viscosity of the kerosene and therefore make it flow more readily through a wick.

To summarize, as a general rule, kerosene is made in such a way that the initial boiling point is between 350 and 400 degrees, although as far as absolute flash point limitations is concerned, it can go as low as around 300 degrees.

Q. Are flash point specifications of any particular importance to-day?

A. They are only of importance in guarding against chance contaminations or getting some gasoline by mistake into a kerosene tank or something of that kind. It is not a specification which ever worries any refiner, but of course, the legal requirements are still there and the products are ordinarily tested as a matter of routine for their flash points. For example, to illustrate what I mean, I think the flash point requirements are not in any case higher than 125, whereas our ordinary kerosene we produce has

a flash point of around 165, which is far above what is really required.

Q. When they were really running for maximum in kerosene that was important?

A. Yes. That then established the lower limit to the volatility of kerosene.

The upper boiling limit on kerosene is determined primarily by its actual behavior in burning in a lamp. The fundamental requirement of kerosene (which, in spite of what we might think is still very widely used in lamps), is that it shall burn in a lamp for a long period without having to trim the wick and still keep a flame of satisfactory shape and height and not smoke up the chimney.

That requirement generally limits the amount of heavy ends which can be put into the kerosene, because the heavy ends tend to increase the average vicosity of the kerosene. I want to emphasize that it is not the heavy ends themselves that matter, but how much they raise the average vicosity of the whole, because if we get too viscous a kerosene when the wick becomes clogged or charred or encrusted [fol. 819] than the flame distorts much more rapidly on account of the fact that it is the average vicosity that controls how much heavy ends you can put in kerosene, it is apparent that the more light ends you put in, the more heavy ends you can also put in, and frequently, even ten per cent of material boiling around 375 degrees will make it possible to put twice as much material in boiling around the upper end of kerosene. So that the absolute upper limit cannot be specified unless we know what the lower limit for that particular kerosene is.

Furthermore, kerosenes which have very little tendency to clog the wick or encrust it can handle somewhat more heavier ends than kerosenes which have a tendency to clog the wick or encrust it.

So we could not state any definite limits as to what the upper end boiling point should be.

Q. How is this kerosene viscosity measured? What is the resultant upper boiling point of kerosenes under present conditions?

A. The viscosity of kerosene is ordinarily measured in a small glass capillary tube, a special instrument known as the thermo-viscometer. That is not generally adopted or standardized by the industry but most refiners that I know

use that at least as a reference standard. That is an entirely different kind of instrument from what is suitable or can be used on the much more viscous lubricating oils, the Saybolt viscosimeter.

The ordinary upper limit thus fixed for kerosene under present conditions, at least for mid-continent crude oil, is from 500 to 575 degrees Fahrenheit. That will vary somewhat, though, with the source of the crude oil.

Q. Now, what would the upper limit be if you were operating a refinery for maximum yield of kerosene, as was done before the motor car demand for gasoline outgrew that kerosene?

A. If we put in this light material into kerosene that has relatively large amounts of material between 300 and 375, which now goes into gasoline, we could then put in kerosenes very considerable amounts of higher boiling material and raise this end point limit by 50 or 100 degrees.

Q. What is it that determines the limits of the primary gas oil cut from the crude stills?

[fol. 820] A. Since gas oil is less valuable than the cuts on either side, it is natural that nothing goes in the primary gas oil cut which will be useful either in the lower kerosene cut or the subsequent pressing distillate cut. In determining the lower limit, therefore, we put everything into kerosene that can possibly go in kerosene under the conditions with which the refinery is operated, and a little more besides, because kerosene is almost invariably treated and redistilled in ore to improve its quality; treated with sulphuric acid. Then it is redistilled and in that redistillation we must leave a certain amount of residue. Therefore, we can put into the first kerosene cut, the primary kerosene cut somewhat more material than is ultimately suitable for kerosene, and then the bottoms from that distillation come back into the secondary gas oil cut. But the primary gas oil cut has been robbed of all material that is suitable for kerosene and somewhat more.

On the upper end there are one of two limitations which determine where the cut shall be made. In the first place, anything that contains any marketable wax must go in the paraffin distillate. We don't want that in the gas oil cut, as paraffin distillate is pressed for the recovery of the wax and the gas oil is not.

The other factor determining where the cut is made between gas oil and the pressing distillate is the necessity of keeping the viscosity of the pressing distillate down to a low enough point so that after chilling it can be put through the presses, as I mentioned this morning. That very frequently takes most of the upper end of gas oil, although again that lighter portion comes back in the secondary gas oil cut.

To summarize, you might say that the primary gas oil cut is what is left over after the maximum desired amounts have been taken out for the primary kerosene and for the primary pressing distillate cuts. And, in some cases, if you are running for maximum kerosene, so that the kerosene has a wide cut, the primary gas oil cut may disappear entirely, and when we stop taking off kerosene you begin taking off material that must go in the pressing distillate. Under present conditions when we are making more gasoline than cakes off the lower end of kerosene and we put more of the upper end of kerosene into gas oil and have a fairly good sized primary gas oil cut.

Q. Now, you have mentioned the subsequent augmenting of this primary gas oil cut by other materials. Will you just name them all at this point?

A. The second gas oil cut includes not only the primary [fol. 821] cut but the heavy ends left from redistilling kerosene and the light ends which are distilled off from the pressing distillate after it has been pressed, and, very frequently, these augmenting materials are larger in quantity than the original primary gas oil cut.

Q. You get some also from re-running the slop cut, don't you?

A. Yes, of course, if the slop cut is re-distilled separately, it will make a certain amount of gas oil. In fact, what you can say is that gas oil is essentially what is left over of the distilled products of crude oil after you have made the maximum yields of gasoline, kerosene, lubricants and wax. It generally boils in the range from 550-750 degrees Fahrenheit.

The Master: The quantities are varied from time to time to suit the demands of the market?

The Witness: Yes, and the nature of the crude, of course.

Q. What is it that determines the limits for the paraffin distillate?

A. I have already explained the lower limit of the paraffin distillate as being determined usually by the viscosity which we want, although sometimes the cut is carried a little lower than necessary to be sure to get all the wax.

The upper limit of the paraffin distillate is determined by the fact that the higher boiling waxes that are present in crude oil are much harder to press from the oil, and therefore you must stop at a certain point without including very much of this unpressable wax in order to get a mix that we can handle in our paraffin presses. The remaining part of the heavy lubricants and the wax—those unpressable waxes—go into what is known as a slop cut which I have previously referred to.

Q. What becomes of the remaining heavy distillates made in cracking crude oil?

A. The slop cut, as I have indicated, is generally re-run, but small amounts are sold for certain purposes and the still wax, a heavy sticky material, which is the last that comes off, which comes out through a separate vapor line is quite generally used in making roofing compounds and such special products.

The Master: And the coke you burn?

The Witness: The coke is sold for domestic fuel; for the most part we burn very little ourselves. It makes a very high grade domestic fuel with very little ash.

[fol. 822] Q. You produced, Mr. Wilson, at various times a number of samples of various products and raw materials. Those are all marked, are they, so that one can identify them with your testimony?

A. Yes, possibly I better just mention the numbers and the names.

The Master: You had better say what is in the case that is produced and that you offer this case as a physical exhibit.

Mr. Wiles: Yes.

The Witness: In this case there are now eleven liquid products and four solid products. In the order of numbers they are as follows: 1, mid-continent crude oil; 2, finished virgin gasoline; 3, finished virgin kerosene; 4, gas oil; 5,

pressing distillate; 6, slop cut; 7, which is in a can rather than a bottle, still wax; and 8, the petroleum coke.

Returning to the liquid samples, 9 is reduced crude from skimming. 10 is cylinder stock. 11, finished cylinder oil. 12, finished pale oil. 13, finished red oil. 14, finished paraffin wax; and 15, finished petrolatum. There are some more exhibits dealing with cracked products which I will later add to this case.

Mr. Wiles: The case in its present condition is offered.

(The said box of samples, physical exhibit, was admitted in evidence and marked "Exhibit 399" and the same is herewith returned.)

A. The best definition I can give of gasoline is that adopted by the American Society for testing materials which states, at least in substance, that gasoline is a refined petroleum naphtha, which, by its composition is suitable for use as a carburetant in internal combustion engines.

[fol. 823] The fundamental requirement for a satisfactory gasoline to be marketed generally is that it gives satisfactory performance in practically every car, under all reasonable weather conditions. It is not merely necessary that a gasoline give satisfactory performance in well-designed cars that are kept in heated garages and operated under warm weather conditions, but practically every car that drives up to a service station, regardless of its age, color or previous condition of service has got to be given a satisfactory product in order for that to be termed a marketable gasoline. Of course, what this really means is that the older passenger cars are really the cars which set the standard which gasoline must meet in order to make it a satisfactory marketable product.

The volatility requirements for gasoline are naturally the most fundamental of any requirements, because we are counting on producing an explosion by means of an explosive mixture of gasoline, vapor and air. In other words, we must vaporize at least enough of the gasoline to make an explosive mixture, and if we are going to use it efficiently we must vaporize most of it under ordinary operating conditions.

The volatility requirements are of three different kinds, depending on three different elements which have to do

with its satisfactory performance under different kinds of conditions. I have already discussed the need for limiting the higher boiling portions of gasoline—in other words, the volatility requirements that have to do with the upper end in order to get fairly complete vaporization and satisfactory distribution of the gasoline in the manifold between the different cylinders.

The second requirement has to do more with the amount of light ends present in the gasoline which must exceed a certain fairly definite amount in order to make starting easy, particularly in cold weather. For example, it is considered generally necessary to have at least twenty per cent of gasoline boiling below 221, and preferably quite a little more than that, for easy starting and satisfactory cold weather operation——

The Master: The use of the term "explosion" in that connection is a somewhat misleading expression, is it not, if the theory of Sir Dugald Clerk—(wasn't it?) who gave the famous deposition in the Seldon patent case, which was a treatise on the internal combustion engine, which theory was that you could take a certain amount of atmospheric [fol. 824] air, and what you desire is to heat that air, quickly, instantaneously, so as to give an expansion in your chambers and the function of the gasoline was simply a quick flashing heater of that air?

The Witness: That is a fair interpretation, I think, yes. It is commonly called an explosion. Of course, it is simply a rapid combustion. It should not be a detonation in the sense that dynamite detonates.

The third requirement, the third volatility requirement——

The Master: Yes.

A. (continued).—for gasoline is one which really acts in the opposite direction; that is, it must not contain too much of the very light ends, particularly what might be called dissolved or liquified gases. Casinghead gasoline, which is made frequently by compressing the natural gas from gas wells or gas coming off from oil wells, is what is known as a very volatile or wild product, because it tends to evaporate very rapidly, light in gravity and highly volatile, and it might be considered that that would make an ideal gasoline; but, as a matter of fact, on account of the presence

of so much light ends, or too much light ends, it tends to boil in the vacuum tank or to form vapor pockets in the carburetor and make a very unsatisfactory fuel. In fact, that very point was in issue in the vase between the Gulf Refining Company and the United States Government, which is decided in 284 Federal 90, and reviewed in 45 Supreme Court 599. It was held therein as established by tests in the presence of both parties that even a 76° Baume casinghead gasoline, which was comparatively heavy for casinghead gasoline, was not suitable for use in a gasoline engine.

When these requirements are balanced against the supply and demand, which exists in a given period of time, they are reflected in certain specifications on the distillation curve, socalled, of the gasoline. That is, there might be a requirement—for example, our present Government specifications require, among other things, that at least 20 per cent of the gasoline shall distill below 221 Fahrenheit, at least fifty per cent shall distill below 284; at least 90 per cent below 392, and all of it below 437. By fixing those points we limit not only the per cent of heavy ends, but also insure the presence, as I indicated, of enough light ends. In most specifications the only means of preventing the [fol. 825] inclusion of too much light material, which I spoke of as the third volatility requirement, is the general specification that the amount of the gasoline recovered in the graduate in distillation, shall be at least 96 per cent. If we have very much of the dissolved gases present they will be lost in the distillation and you have less than 96 per cent recovered in the flask. For example, with casinghead gasoline frequently around only 90 per cent is recovered in the flask. The only reason that more definite specifications are not present is that ordinarily no one wants to sell casinghead gasoline when they can use less valuable heavier products with it and still make a marketable gasoline, so there isn't much temptation to add too much light ends present.

When we come to aviation gasoline, however, which must be a light product, there is more danger of having too much light ends; also there is more danger of forming gas pockets and causing a serious accident. For this reason aviation gasoline has a specification that the initial boiling

point shall not be below a certain temperature, I think it is not below 135° F., although I am not positive on that.

It is, of course, necessary, that the gasoline shall be free from sulphur, or corrosive sulphur compounds, which tend to act on the iron, or copper, or brass, in the fuel feeding system, including the vacuum tank, the carburetor and the manifold, and definite requirements are put in, in order to prevent any danger of that happening.

It is also true that even though a gasoline is free of corrosive sulphur compounds, if sulphur in any form is present in gasoline, when it burns it forms a small amount of sulphuric acid, and that, if it gets down into the crank case and dissolves in the water which may condense there, makes a rather bad corrosive condition. So that the total amount of sulphur in gasoline has to be limited very sharply, generally below one-tenth of one per cent.

The color, odor and gum forming tendencies of gasoline are of considerably less fundamental importance than the other requirements which I have mentioned. Nevertheless, if they are not kept in mind they will lead to complaints and increased sales resistance, and, therefore, they cannot be lost sight of by anyone who is marketing any considerable amount of gasoline.

[fol. 826] Of course, color, by itself, is not of particular significance. That is, you can dye gasoline any color, with a suitable dye, and it would make no particular difference. But it is true that the well refined gasoline does have a good color, and remains fairly stable in color, whereas, most of the highly unstable compounds tend to produce color.

Furthermore, the fact that most of the gasoline—or a great deal of the gasoline is sold in visible pumps, so-called, makes that one property of gasoline which a customer does observe, and he considers it more or less vital that his gasoline should be well refined to a good color, and complains if he doesn't get it.

The odor is probably even less significance from the standpoint of operation, than the color, and yet if the odor is off it leads to a surprising number of complaints. It must be remembered, of course, that about half the cars of today still have the gasoline tank either under the front

seat or in the cowl, and any spillage from filling or breathing, when the tank warms up, sends vapors where the driver can notice them very perceptibly.

Requirements for the absence of any considerable quantity of gum forming constituents, are coming into more prominence very recently, than they formerly had, the reason being the increasing amount of cracked gasoline, some of which is not so well refined as it might be, and particularly cracked gasoline made by certain high temperature processes, which give somewhat better anti-knock value—that is, less tendency to cause knocking in a carboned up engine—but have the characteristic unstable compounds [fol. 827] present that are formed by high temperature cracking.

The best evidence indicates that these gum forming constituents do no particular harm as long as the gasoline is fresh. In other words, they are the materials which, on storage, will absorb oxygen from the air and throw down, in time, gums.

As I said, if you used the gasoline fairly fresh, that did no particular harm. They are then gum-forming constituents, and not gums. But if it stands around in the tank, in a heated garage, or, in process of being marketed, or stored, over long periods of time, it will throw down deposits—characteristic gummy deposits, and while one tank full might not—or several tanks full might not do any considerable damage, in time they are likely to clog the gasoline feed system, or carbureter, and form a deposit around the intake valves of the engine, and make trouble.

It is, therefore, becoming more and more recognized by the better refiners that they must watch that feature. There are two or three cases where refiners who have not watched that feature, have gotten into very serious trouble, and had to take it into account.

The particular specification that refiners use to prevent getting too much of these gum-forming constituents, differs in different refineries. It is not a standardized feature that is always present in specifications, though I think it is in a good many of the specifications of efficient refiners.

As a matter of fact, the color requirement, itself, is a fairly good protection against gum-forming constituents, because if we have those gum-forming constituents, the

first thing that happens is that the gasoline goes off-color, and continuing the same type of change, eventually results in the precipitation of this gum.

The Master: Is that held in suspension or deposited in the container, at the bottom?

The Witness: It depends on the amount of circulation. If there is much circulation it will be held in suspension, but if it is allowed to stand still it will generally be deposited on the bottom of the container.

I have some samples that I will put in, shortly, which will show small amounts of gum settling out, in a short time.

Q. What is the test commonly used to detect the presence of gum-forming constituents?

A. The ordinary test is to take a hundred cubic centimeters of the gasoline and evaporate it slowly on a steam bath, in a copper, or glass dish, until it is substantially [fol. 828] dry. This slow evaporation in the presence of air, and at an elevated temperature, accelerates the formation of the gums, which would otherwise, take place more slowly in storage, and the gums are dried fairly thoroughly, and the dish weighed, and deducting the weight of the dish, itself, gives the numbers of milligrams of gum which are formed there, which, in ordinary straight-run gasoline, is generally below five milligrams of gum, whereas, with unrefined cracked gasoline, you would get from one hundred to a thousand milligrams of gum.

This, however, can be eliminated by refining methods which we now know, with more or less loss, of course, involved in doing so.

I may add that the test is not particularly well standardized, and details in running the tests will give variations in results, particularly as to whether you use a copper or a glass dish—the copper dish tending to give higher and somewhat more erratic results.

Q. Are there any other properties which a satisfactory motor fuel should possess?

A. Yes. Of course it is necessary that the gasoline be free from water, or suspended matter, or undissolved material, which would deposit a solid when the gasoline evaporated. These are frequently included in specifications.

In addition to that there are a number of requirements which we frequently lose sight of, because practically any gasoline meets them. But when we consider various substitute motor fuels, we have to keep them in mind as being really fundamental requirements for a satisfactory motor fuel.

For example, one requirement is that the material should not freeze even in very cold weather. That, for example, rules out pure benzol, which freezes at a little above the freezing point of water.

Another requirement that must be kept in mind, is that it should not dissolve shellac, because a good many of the older cars have cork floats which are covered with shellac, in their carburetors, and if you use pure alcohol, or anything like that, the shellac dissolves off and the cork sinks. The use of considerable quantities of alcohol in motor fuel, also has the disadvantage in that it tends to loosen up gum deposits which have formed during the long period of using gasoline. Unless the percentage of alcohol is kept quite low it will also affect the shellac, as I have mentioned.

It is also important that any material added to the fuel, [fol. 829] should be readily extracted by water, for very frequently, in the bottom of storage tanks there will be a deposit of water, due to various conditions which may take place, and particularly around a refinery, and if the added material is quire soluble in water, it will be quite likely to be taken out of the fuel in this manner. The gasoline draw-off, of course, in the storage tank, is above the water, so that water will settle ordinarily and do no harm, and simply be run to the sewer occasionally, as it accumulates.

It is also necessary, for the purposes of motor fuel, to avoid certain products which tend to give more corrosive compounds, on burning, than does ordinary gasoline.

Another requirement, and, of course, the fundamental thing that we are paying for in a motor fuel, is the amount of heat which will develop when you burn a gallon, or pound of material. Take a product known as wood alcohol, which has been suggested for use as motor fuel—it has only around two-thirds the heat of combustion, or the ability to develop power in a given engine, the gasoline has, and, therefore, would not be a satisfactory motor fuel, and, of course, would require a rather radical change in design

of engine, to use it efficiently. Of course small amounts could be put in without making a great deal of difference.

Those, I think, cover all the important requirements for a satisfactory motor fuel.

Contrary to very general opinion the fundamental requirements for satisfactory motor fuel have changed very little, mainly because we still have some 1912, or not much more recent, cars to serve from the filling stations.

In addition to the fact that there are many old cars of the period from 1912 to 1916, which still have to be taken into consideration, a lot of the cars have made very little improvement in their fuel handling system, since that time. That is particularly true of the cheapest cars, and as I indicated, it is the poorest cars that still determine the fundamental requirements we have got to meet, and that means that we are putting out a better grade of gasoline than——

The Master: You refer to a well known, popular car?

The Witness: Well, I don't like to talk about our best customer. There are many higher priced cars, of course, which could use a much lower quality of motor fuel, and [fol. 830] still give satisfactory performance, but that doesn't make it a marketable gasoline.

The best illustration of what I mean is the fact that the real change in the end point—the real increase in the end point of gasoline, which people talk so much about, took place almost entirely between 1912 and 1920, during which period there was very little improvement in the fuel systems of motor cars.

Since 1919, or 1920, on account, and as a result of the increase in the end point, which was caused by the action of the law of supply and demand, more manufacturers have given more attention to, and have made very considerable improvements in their fuel handling systems. During that period, however, when the cars have been improved most, the gasoline has actually dropped somewhat in end point, so that today, in general, the gasoline is more volatile than that in 1919.

So that the improvements in the fuel handling systems have been the result of, rather than the cause of, the increase in the end point of gasoline, since 1912.

Q. What is the fact as to the presence of a satisfactory gasoline in marketable kerosene now, or at any time?

A. Well, it is true, as I pointed out, that the material boiling between three hundred and about four hundred and fifty degrees, can go either into gasoline, or into kerosene, it is, nevertheless, a fact that in the process of making kerosene marketable by distilling off the lighter ends, the very materials which are necessary for gasoline are eliminated. They were taken off in the benzine cut, which refiners generally made, before gasoline became a major commodity.

It is true, of course, that the legal requirements for flash point of kerosene, did not come into effect until the latter part of the last century, but it has always been recognized that kerosene should be free from too much light ends, and be safe to handle, even around an open flame. To illustrate that point, I took a sample of pressure distillate, containing both gasoline and kerosene, and distilled off the light ends until I got just a ninety degree flash, which was as low as anyone could consider safe—that is, this kerosene flashed at ninety degrees, and therefore contained much lighter material than would be considered safe to market today, but when taken and redistilled, even the first 2 per cent of lightest material was nowhere near volatile enough to be a satisfactory gasoline. So that in my opinion neither today, nor at any time, has there been any appre- [fol. 831] ciable quantity of marketable and satisfactory gasoline in kerosene.

Q. Now, you have already stated the minimum temperature for cracking. What effect does the raising of the temperature have upon the rate and nature of the cracking reaction?

A. Of course, in stating these reactions of cracking, they are very complex, and it is very hard to keep everything constant. To the best of my knowledge and belief, however, for two or three hundred degrees above the temperature at which cracking begins, the nature of the cracking does not change very much by increase of temperature, but the rate of cracking goes up very rapidly—approximately doubling for every eighteen degrees Fahrenheit. At temperatures above 900 or a thousand the nature of the cracking reaction does change very considerably, with a yielding of more unsaturated, unstable, and aromatic types of hydro-

carbons, and a smaller amount of the paraffin types of hydrocarbons which resemble those in our ordinary straight-run gasoline.

Q. In this upper range, above 900 or a thousand, what about the rate?

A. I don't know how temperature affects the cracking quantitatively, but there is no doubt but what the higher the temperature is, the faster it goes. It is quite hard to keep track of the temperature, at these very high temperaturs, since most of them are in tube cracking processes, where the temperature gradually increases as you pass through the system, and we cannot get as good a line on rates as at the lower temperatures characteristic of bulk distillation processes.

Q. So that it increases, but you don't know how fast in quantity?

A. No.

The aromatic hydrocarbons are a class of hydrocarbons very seldom found in petroleums—in fact, I don't know of them being found to any appreciable extent in American petroleum, although they are in certain other petroleums, and probably among some of the thousands of compounds in crude oil there may be some aromatic hydrocarbons there. They are not classified as either saturated, or unsaturated hydrocarbons. They are ordinarily considered as a separate, third class, characterized by a peculiar ring structure, such as is in benzol. This ring has alternate [fol. 832] double and single bonds, instead of having all single bonds, as in the naphthene rings, but the double bonds do not have the characteristics of the double bonds in the straight chain hydrocarbons, and they are relatively stable.

The Master: Why the term "aromatic"?

The Witness: They all have rather distinct and characteristic odors, and, of course, other aromatic hydrocarbons are made, generally from coal tar, and they are the basis for dye-stuffs and synthetic perfumes, and all those industries. They are things which we ordinarily consider very little in the petroleum industry, because we have so little of them, except when made by those very high temperature tube cracking processes, which are not used in the oil refining industry to any marked extent. They are used in the

gas making industry, and the oil gas tars contain considerable quantities of these aromatic hydrocarbons, made by cracking oil at very high temperatures.

The Master: Let me ask you—is it understood that the behavior of atoms, when released from a molecule, is not definitely and certainly known?

The Witness: Absolutely. They behave in so many million different ways that all we can talk about is that at certain temperatures, with certain starting materials, we get certain relative proportions of the different type of reactions.

The Master: You only know their behavior empirically, then?

The Witness: Yes, sir. It is not predictable in any sense.

So far as I have been able to determine pressures, at least up to around 800 pounds, and at temperatures below 900 degrees Fahrenheit, have very little effect on the course of the cracking reaction itself,—that it, as it does not, in itself, affect the nature of the changes.

The most important function which pressure has is to keep materials in a liquid form, when, otherwise, they would form vapors.

In some types of cracking processes, that is, as I shall point out later, of fundamental importance, and in other types of processes it makes very little difference.

Another function which pressure has, is in compressing the vapors, making them denser. In some processes that is quite important, and in others it is not of so much importance.

At very high temperatures there are those who maintain that pressure does have a very great effect on the products [fol. 833] made. It is very easy to fool yourself in that, because when you increase pressure you generally change the nature of the process in other ways, at the same time. For example, it is very hard to put pressure on a given piece of apparatus and not change the temperature to which the oil is heated, or the time that it is in the system, and unless you control those things you cannot know what the effect of pressure is, or what the effect is of the other things that have changed.

Q. You have already defined cracking in a chemical sense. Now, how would you define a cracking process?

A. As the term is generally used, a cracking process is one where the splitting apart, or joining together of the hydrocarbon molecules, proceeds to an extent sufficient to form a new product in commercial quantities. In other words, mere minor, or incidental cracking, in the distillation of crude oil, does not make that a cracking process in any proper sense, or in the usual use of the term, but only when there is production of commercial quantities of a new product.

The Master: Do we understand that in the low pressure distilling of gasoline the molecule has not been disturbed, and is the same molecule that was in the original organic substance?

The Witness: Yes, sir. Though in the latter part of coking distillation, of course we do have a cracking process which breaks up the heavy lubricants and makes gasoline and kerosene.

I find it very convenient to divide cracking processes into a number of distinct groups, in accordance with the general principles under which they operate, and which control them.

There are many very important and fundamental differences between the different types of cracking processes. I generally consider seven different types—six, or possibly seven different types of cracking processes.

The first might be termed catalytic—catalytic cracking processes, where the oil is treated with certain chemicals, such as aluminum chloride, or other catalysts, at generally somewhat elevated temperatures, and mainly on account of the presence of those chemical materials there are changes in the joining together, or splitting apart of the molecules which produce commercially useful results.

The outstanding commercial example of this action is the McAfee process for making gasoline with aluminum [fol. 834] chloride as a catalytic agent, which the Gulf Refining Company has, and I believe still does, practice to a considerable extent.

I do not include in that catalytic process class certain processes which claim more or less catalytic action. There are a good many of the prior art that put iron filings or Fuller's earth, or any one of a dozen things, into their reaction chamber, or tube, and claim that they improve the

results. But, as a matter of fact, most of those are substantially, or entirely inactive, and the cracking is produced mainly as the result of temperature. So that I consider them mere thermal cracking processes, rather than catalytic processes, where the chemical is really the main thing.

The second type of cracking process is the electric cracking process, where either the oil, or vapors from the oil, are subjected to an electric arc, or some such electrical discharge. There are a number of those electrical cracking processes described in the art, and there is no doubt but what rather unusual types of changes can be produced under some circumstances. None of them have ever been economical enough to become commercial, and I don't think any of them have ever operated, except on a semi-commercial scale for a short period of time.

Again, I might say I do not include in electrical processes, properly, those in which the electrical energy is used merely as a source of heat, as in some of the processes, but where the vapors are passed through an arc, or silent discharge, or something of that kind.

The third type of cracking process is the one which is doubtful whether it should be given a class, or not. It is based on the claim of certain inventors that by moving at very high velocity it is possible to break up the molecules and produce cracking apart from temperature.

I think it is doubtful whether that is a type of cracking process, or a type of cracking patent which is quite a different matter.

The four remaining types of cracking processes have one feature in common, and that is that the change is produced mainly as the result of the temperature to which the oil is heated. Therefore, on account of this similarity they can be broadly referred to as thermal, or pyrogenetic cracking processes. But, as I shall point out later, they differ very greatly in the nature and character of the product produced, and the principles that govern their operation.

The four thermal processes are, first, bulk distillation by atmospheric pressure which is characterized by the fact [fol. 835] that a large body of oil is held at a cracking temperature and pressure for a very long period of time—a matter of hours or days. That is typified in the latter part of the ordinary coking method of running crude oil —that is, bulk distillation without pressure would include that class of operations. Of course, that is the one which

was by far the most widely used prior to 1912, and even today is very widely used.

Q. Why was that so common?

A. Well, it was very simple and easy to carry out, and, of course, it did produce considerable quantities of kerosene, which was one of the things most desired from the crude oil, in those days. Of course that method of cracking was only applicable to materials which boiled higher than gas oil, because everything, in the ordinary gas oil range and lower, distilled out before appreciable cracking began to take place. It was only the heavy products that stayed in the still, that were cracked to any considerable extent.

What I term the second class of thermal cracking processes—bulk pressure distillation processes—are similarly characteirzed by being a very slow distillation. But in this case we hold a large body of oil at a cracking temperature for a long period of time, a matter of hours and days, but under substantial pressure, and distill out of the system the products somewhere near as fast as they are formed, letting the unchanged products stay back for a longer time, under cracking conditions. The pressure, of course, raises the temperature at which given products can be held liquid, and, therefore, tends to increase the cracking rate and makes it possible to crack lighter materials than in a still without pressure.

The third type of thermal cracking process which I shall refer to as tube cracking processes, is a class that includes a great many of the prior art processes, where the oil is passed through a long narrow retort, or tube, which is heated, generally to a bright red heat, and the stream of oil is passed through, and undergoes progressive evaporation and decomposition. Of course, in some cases it has been vaporized before it enters the cracking system.

In the tube cracking process we pass the material through the long tube, and it gradually vaporizes, but both the liquids and the vapors, and both the uncracked and cracked materials go along together, and are subjected to cracking for approximately the same length of time.

The Master: In the bulk system was there a continuity of flow?

[fol. 836] The Witness: There may be and there may not be. In none of the earlier forms was there a continuity of flow. That was one of the more recent developments.

The tube cracking processes, on account of requiring a shorter time, necessitate the use of a higher temperature, in order to produce a reasonable amount of cracking, and they characteristically have operated with temperatures considerably above those used in bulk pressure distillation, and have a time of contract that is measured in minutes, rather than hours and days.

They all have some pressure at the inlet of the coil, and in some cases they carry pressure clear through the coil. But pressure doesn't make a great deal of difference in the tube processes, as compared with bulk pressure distillation.

The fourth class of thermal cracking process is what I call flash, or film cracking, in which the cracking takes place in contact with very highly heated surfaces. That is illustrated in the gas enriching art, where oil is sprayed on very hot fire brick surfaces and is almost instantaneously vaporized and cracked and leaves the system. These are characterized by extremely high temperatures and a large production of gas, and the time of contact is measured in seconds, even, rather than minutes, and there is only a film of oil, if any liquid at all, in the cracking zone.

The Master: They do not come within the purview of cracking for gasoline—they are simply gas processes?

The Witness: Yes, sir. They might be modified, but, commercially, they are only used for making gas, as far as I know. Of course, in making gas you do make fifteen to twenty per cent of liquid products, unavoidably. They are largely aromatic hydrocarbons, in this high temperature type of cracking.

Q. Some of those were cited here in this case, by the plaintiff, were they not?

A. Yes, they were.

The Master: Do you refer to the toluols as benzols in the gas art?

The Witness: Yes, sir, those are the same characteristic aromatic hydrocarbons that I spoke of as being formed either by the distillation of coal, or, by the high temperature cracking of oil.

Q. Now, do the tube and flash cracking processes involve an evaporation step? If so, just how do you distinguish them from the bulk pressure distillation process?

[fol. 837] A. Yes, they do involve, at some stage of the process, the evaporation of the oil. But they do not have the large body of oil held at a very low cracking temperature for a long period of time. They bring about their action, to a very considerable extent, in the vapor state, and characteristically have only a small stream or film of oil under treatment at any particular time.

Q. Now, you have referred to six or seven different methods of making light and heavy oils by cracking—do you consider them essentially similar in kind?

A. No, they are essentially different in kind. Of course you can point out some similarities in almost any two of them, but the fundamental principles back of them, and the kind of products that are made, and the kind of stocks that are suitable, and the temperatures and pressures required, are all quite different for the different types of processes.

Q. The fact that a certain temperature or pressure worked with a given charging stock for one—would that give you any information about the others?

A. In general it would not.

Q. Do you find any serious difficulty in classifying various references cited by the plaintiff, in the groupings that you have given?

A. No. There are only two or three places where there is any question, at all, on account of the complexity of the process, or the vagueness of the disclosure. There are places where it is a little difficult, to be sure, but when I come to discuss those processes I will make clear the reasons for the classifications which I decided upon as proper.

Q. Has the plaintiff cited any references which belong to the first three classes—that is, the catalytic, the electrical and the impact methods?

A. Not properly speaking. Of course there are some of their citations which refer, as I indicated, to the use of catalytic materials in what are really thermal cracking processes, where the catalyst is really not a catalyst.

Q. Those are such as the Noad patent, and the Testelin and Renard patents?

A. Yes sir.

Q. Now, confining yourself to the remaining four types of cracking processes, what are the principal points in which they differ from each other?

A. In general they differ in the kind of stock which they require to give satisfactory operation, and the tem- [fol. 838] perature at which they operate, and the times of contact required to bring about the desired changes; in the function of pressure, and the necessity of correlating temperature with pressure, in order to obtain a useful result, and the desired yield of products.

Now, of course I don't mean to say that every one of those processes differs from every other one in all those respects, but, as a rule, in at least three-fourths of those respects it does differ from all the other processes.

As to the stock requirements of the different types of processes, of course it is possible to find in the prior art, a wide variety of disclosures, some of which—many of which simply will not do what is claimed for them.

If we mean, by stock requirements, not what somebody says should be used in a certain process, but what can really be used and produce anything approaching commercially useful results, we have very definite limitations.

For example, as I have already pointed out, this bulk distillation without pressure, can only be applied to stock boiling above about 700 degrees Fahrenheit.

These reduced crudes are handled without difficulty, as these stills are not under pressure, and they can stand a very considerable amount of coking without danger.

In the bulk pressure distillation processes, on account of the use of pressure it is possible to work with lighter stocks and get them to crack, and it might, therefore, be thought that the use of pressure would greatly increase the number of stocks that could be used for cracking. As a matter of fact, however, pressure works the other way, because just as soon as we put pressure on a still, we lose the tolerance for handling coke. As soon as we put pressure on it, the much greater hazard involved, in the extra strain on the metal means that the tolerance for coke is reduced to a mere fraction of a per cent., and so, as a matter of fact, there are comparatively few stocks that can be used on a large scale, if any considerable amount of cracking is to be done under pressure.

The fundamental differences between two types of processes—even two types of processes as similar as bulk distillation without pressure, and bulk distillation with pressure, is very clearly brought out by the fact that reduced

[fol. 839] crude can be used in bulk distillation without pressure, where gas oil cannot, whereas, in bulk distillation with pressure, where gas oil can be used, as we now know, but reduced crude cannot, because it deposits entirely too much coke for safety. Unless this very careful selection of stocks for bulk pressure distillation is carried out, and some of the expedients developed since 1912, are employed, the hazard of having a large amount of oil under a high pressure, in a still that has the slighest danger of depositing coke and getting a hot bottom, is so great that the refiner simply could not consider it as a feasible process.

As to the stock requirements in the tube cracking processes, it, in general, is suited to a wider variety of stocks than almost any other process—particularly as far as their boiling point is concerned, because you can handle very light stocks and crack them in tube processes, even though they are mostly vaporized, or you can handle very heavy stocks.

Tube cracking processes, characteristically, although not invariably, run red hot—that is, they allow the tubes to become red hot. Because the thickness of the wall, in comparison with the size of the tube, is so much greater than in a shell still,—thus lessening the strain on the metal—it can stand a great deal more coke, but even then they cannot crack a very dirty stock, they get into trouble from clogging, and other serious difficulties.

In the flash cracking processes the stock requirements are somewhat similar to those in the tube cracking processes—that is the boiling point doesn't matter very much, but you cannot have too much coke residue, because it is liable to clog up the passages in the flash cracking apparatus.

In bulk distillation without pressure, cracking generally starts at an appreciable speed—at around 725 degrees Fahrenheit, and continues to the maximum temperature reached toward the end of the coking operation, which is probably in the neighborhood of a thousand degrees Fahrenheit, or higher.

The times involved are generally a matter of hours or days.

In bulk distillation processes under pressure, the times are generally similar, or somewhat shorter, and the cracking begins at about the same point—around 725 degrees—

but the operation is seldom carried on to a temperature above around 820 degrees.

[fol. 840] In fact, all the commercial bulk pressure distillation processes with which I am familiar, operate between 720 and 820 degrees, although they can be carried out at slightly higher tempeartures, on a smaller scale.

The tube cracking processes ordinarily operate at temperatures from a thousand to fourteen hundred degrees Fahrenheit, although in some cases they are carried out at lower temperatures.

This, of course, refers to the maximum temperature, and not the average temperature of the process, because they are heated to progressively different temperatures. I might add that in any ordinary bulk pressure distillation processes, the temperature varies very little, during the cracking period of the operation; it remains almost constant, whereas, in bulk distillation without pressure, and in tube cracking, we have a gradual rise in temperature. In tube cracking processes the time of contact is usually a matter of minutes.

In flash cracking processes, the temperatures may be anywhere between a thousand and two thousand degrees Fahrenheit, and the times of contact are generally measured in seconds.

In bulk pressure distillation, the pressure, and particularly the correlation of pressure with temperature, are of absolutely fundamental importance in determining just what is cracked, and the character of the products produced.

To illustrate that, suppose we want to crack a mixture of kerosene and gas oil and lubricating oil, to make gasoline, essentially.

As any given cracking temperature—say 750 degrees, if we find that suitable for a particular product—there is only a very narrow range of pressure which will hold the kerosene and gas oil in the still, and allow the gasoline to distill out of the still, as fast as it is formed.

Both of those things, of course, are desirable things to bring about in a bulk pressure distillation process. If we use a pres-ure that is higher than that necessa y to hold the kerosene in the still, we will hold considerable amount of gasoline in the still, thereby subjecting it to further cracking, destroying the gasoline which we want, and mak-

ing an excessive amount of gas. If we use a pressure a [fol. 841] little lower, then we allow the kerosene to distill out at that temperature, along with the gasoline, and then we fail to crack the kerosene, which is what we wanted to do.

So that, as I indicated, it is of vital importance in determining what is to be cracked, and the nature of the products to be produced, that we properly correlate the temperature with the pressure, in these bulk pressure distillation processes.

The Master: Let me see if I understand that. We will assume that there is a pressure apportioned to a temperature which will hold kerosene liquid, but will not hold gasoline liquid—it will vaporize and go over and be condensed again?

The Witness: Yes, sir.

The Master: And that is the relation that has to be maintained in order to carry over the gasoline, and not the kerosene?

The Witness: That is the idea, exactly.

Mr. Wiles: With that particular stock——

The Master: Yes, exactly.

The Witness: So that, as I indicated, the correlation of temperature and pressure, in this type of process, is an absolutely vital matter, and of absolutely vital importance.

When we come to tube cracking processes, pressure has a comparatively unimportant function, and there is no real good in close correlation of pressure with temperature.

It is true that the pressure that we have on the system determines the exact point at which a given product will vaporize as it passes through the tube, but it all has to pass through the tube, whether in liquid or vapor form. So that it is not a matter of much moment what pressure we use.

In other words, we can crack a given product at a high pressure and a low temperature, or at a low pressure and a high temperature in a given amount of time.

The importance of the correlation of the temperature and pressure is almost negligible in the tube cracking processes. It cannot affect the nature of the product because, of course, everything comes out together instead of just

taking off gasoline and leaving back what we want to crack, as we can do in a process of bulk pressure distillation.

In flash cracking processes pressure is of comparatively minor importance. In the first place, it is seldom used because the temperatures are so high it is hard to get material which will stand pressures at those temperatures, but if it is used, it doesn't reduce vaporization—because va- [fol. 842] poration takes place almost immediately, and if it is to have any effect it is the effect on the nature of the cracking reaction, which as I stated, some people claim does affect the nature of the cracking reaction above a thousand degrees. If that is the case you really have three different functions of pressures in these three processes; one, the correlation of the temperature and pressure in bulk pressure distillation, which determines what is to be cracked and determines what is to be gotten out. In the tube cracking pressure increases the density of the vapors and the cracking is done for a somewhat longer period of time without affecting what comes out or what stays in the still.

In the flash cracking processes, if it has any real function, it is in affecting the nature of the cracking reaction, possibly by affecting the equilibrium.

The Master: We are not going to be concerned here with the flash cracking processes at all.

The Witness: It has been cited in a few cases, but I simply call attention to the fact that they are not part of this bulk pressure distillation art, which mainly concerns us.

The Master: As a matter of fact, we are only going to be concerned here with bulk pressure distillation and tube pressure distillation.

The Witness: That is correct.

Mr. Wiles: This is merely to establish in a preliminary way that flash references haven't got anything to do with the case.

The Master: Oh yes, indeed.

A. The bulk distillation without pressure produces mainly kerosene and gas oil as products of cracking. There is, to be sure, a small amount of gasoline and of course gas and coke are also products of this cracking operation.

Bulk pressure distillation can make a wide variety of products, depending upon the correlation of pressure and

temperature. That is, the bulk pressure distillation operations can be carried on primarily to produce kerosene, as was the case in practically all the prior art processes, but you can change that correlation and make them produce gasoline. For example, lowering the temperature for a given pressure makes it produce gasoline rather than kerosene. That is opposite to what you might expect a priori; the harder cracking, the more complete the cracking, produces gasoline, and in the bulk distillation process at a [fol. 843] given pressure this can only be done at a lower temperature than is required to produce kerosene. You want a high temperature to produce kerosene and distill that out as fast as formed. If you want to produce gasoline at that pressure you must keep the temperature so low that the kerosene will stay in there until it is cracked to make gasoline.

Further with regard to the products of bulk pressure distillation, gasoline or kerosene that is made in that way is usually of a rather high degree of saturation for a cracked product. That is not more than 20 per cent or so of products soluble in sulphuric acid. Another important product of this type of cracking, is the pressure tar, for reasons I will indicate later. Bulk pressure distillation may be only carried on to a certain stage since products in the still are synthesized, heavy products made in the reaction to such an extent that coke begins to deposit rather rapidly. Then the reaction is stopped, and that pressure tar is from 30 to 60 per cent of the products, and is found to contain a lot of new material which has been synthesized in the course of the cracking reaction. Ordinarily bulk pressure distillation is not allowed to produce more than a tenth to a quarter of a per cent. of coke, although in some of the more improved processes they can tolerate greater per cents. of coke than that.

In the tube cracking processes the products—the gasoline-like products, particularly of the high temperature processes, (which most of the tube processes are) are highly unsaturated and aromatic in their composition, generally very yellowish in color and frequently fifty per cent soluble in strong sulphuric acid, although that is not invariably the case in tube cracking processes. But the character of the product is much more different—the character of the gasoline product is much farther away from the ordinary

straight-run gasoline than is the product of bulk pressure distillation.

Furthermore; on account of the fact that the temperatures are generally higher and the fact that the gasoline is not removed from the cracking zone as fast as formed but is permitted to continue on through the higher temperature cracking zone, gas losses are almost invariably high in these tube cracking processes, if any considerable amount of gasoline is made. That is, where the ratio of gas and gasoline—where you make in bulk pressure distillation only ten or fifteen per cent as much gas as gasoline, in the tube [fol. 844] cracking processes, in many cases, you make two-thirds as much gas as gasoline, due to what we call secondary cracking, that is re-cracking of gasoline, which has once been formed and then of course, when you crack that you get a considerable amount of gas by splitting up the molecule still further, and also part of that tends to go back and form heavier material at the same time.

The Master: Those gases are fixed gases that will not liquefy, are they?

The Witness: Well, they will liquify if you put enough pressure on them; practically all. But they won't condense at ordinary temperatures and at ordinary pressures. If you put pressure on them you can liquefy them and recover them, and that is usual in certain cases. Pintsch gas, for example, is that type of product and held under pressure.

Q. If at atmospheric pressure and temperature what would be the case?

A. At atmospheric pressure and temperature, they would not be liquified and hence not available for marketing as gasoline. They would be the wild type of products which I spoke of wanting to keep out of gasoline in order to make it perform well. There is also, of course, a certain amount of tar formed in these tube cracking processes, the character and amount depending on the stock started with and the extent of the cracking and the nature of the cracking, but in general they are quite different from the product formed in the long, slow stewing action characteristics of bulk pressure distillation.

In some cases in tube cracking processes the tar is formed, but does not escape from the still but is converted to coke right in the still—not in the still but right in the

pipe, and we have only vapors coming out of the pipe. In no case do we have a product closely resembling the characteristic pressure tar of bulk pressure distillation.

In the fourth type of process, the products are always of the aromatic, more or less unsaturated nature, characteristic of high temperature cracking and the liquids are generally only 15 or 20 per cent of the total amount of the charge. There is quite a little coke formed and the rest of the product is gas.

Q. Have you samples of the distillates made by these thermal-cracking processes?

A. Yes, I have here a sample of the crude pressure distillate from a bulk pressure distillation process as practiced [fol. 845] commercially, and a sample from a fairly typical tube cracking process. Some tube cracking process samples would be better and some would be worse than this, but this is from a fairly typical tube cracking process. Both of these are in the state as they come from the cracking process without any refining. You notice in the bottom of both there are small amounts of gum deposited, considerably more in that from high temperature tube cracking. You will also notice quite a difference in the characteristics of those two types of products. Both of those, particularly the tube-cracked-product, will probably get worse and worse in color and tend to deposit more gum over a period of storage.

Q. Now, for the record will you please state what their present color is as nearly as you can, if they are going to go off?

A. The pressure distillate, I should say, has a color in the general neighborhood of 15 on the ordinary Saybolt scale. That is a little lower than would be marketable gasoline but it will of course get worse on standing.

The tube-cracking pr-duct, sample No. 19, has an orange color, and has a distinct layer of gum on the bottom of the bottle.

The Master: Dark amber in color, isn't it?

The Witness: The Master describes it as a dark amber; yes.

The Master: Is that a fairly good description of it?

The Witness: Yes, dark amber is better than orange. It does not have the reddish tinge.

Q. Now, were these effects of temperature and pressures and the characteristic limitations in various types of processes which you have just been over, appreciated in 1912?

A. No, it is only the very extensive work, that has been carried on in the last 15 years or so, that has really brought out these fundamental facts regarding the importance of different elements in cracking. There was a great deal of misinformation and misstatements current in the literature until very recently. Even as simple a point as the quantitative effect of temperature and the real function of pressure in high pressure bulk distillation processes was, I believe, first adequately described in a patent which was issued since this case began, which was filed in 1919 and [fol. 846] issued September 15, 1925, to Humphreys, Rogers & Paulus of the Standard Oil Company of Indiana. I have here a copy of this patent.

The Master: You mean that had never been adequately described before in the history of the art until that patent?

The Witness: So far as I have been able to find the quantitative effect of temperature on the rate of cracking and the precise function of pressure in this kind of cracking had not been described before that time. This set forth the real, proper conditions and the fundamentals of high pressure, high temperature bulk pressure distillation processes as very extensively practiced today.

The Master: Does it accord with your own views?

The Witness: Yes.

Mr. Wiles: The patent is offered as Defendants' Exhibit 400.

(The said patent so offered, was marked "Defendants' Exhibit 400" and the same is herewith returned.)

Mr. Wiles: Now, just continue your answer.

A. (continued). As to the extensive prior art disclosures they are full of very misleading and incorrect statements regarding the fundamental principles of cracking.

The Master: In the light of modern or recent researches?

The Witness: That is right.

Q. Now I wish you would point out some of these statements in the plaintiff's exhibits which show the general opinion prior to 1912 as to the nature and mechanism of cracking without pressure in bulk distillation processes?

A. Even with regarding to the comparatively simple case of cracking in a bulk distillation process without pressure, which was so widely practiced and where one would expect what was really happening would certainly have been found out, the misconception was widespread and its true nature was not understood. The most prevalent opinion in the prior art was along the lines mentioned by Redwood in his treatise on petroleum, which was probably the best known treatise on the subject in the entire art.

He states in this book, this treaties on petroleum, a part of which is cited in Plaintiff's Exhibit 107 on page 1384 of the book of exhibits as follows:

"The cracking process practically consists in distilling oils at a temperature higher than the normal boiling points of the constituents which it is desired to decompose. This may be brought about by distillation under pressure or by allowing the condensed distillate to fall into the highly heated residue in the still."

[fol. 847] Now, according to this disclosure the only things which could be cracked in a process of distilling without pressure are the things which boil below the still temperature, and of course the only way to get these even *even* momentarily above their boiling point without pressure is to condense them and drop them back into the still.

The real facts, of course, are that the only things which crack to any appreciable extent, are the things which boil higher than the still temperature and therefore remain as liquid bodies in the still.

Redwood further says on the next page (I am quoting from page 1385) with regard to the discovery of the cracking process, the last sentence in the first full paragraph:

"As the results of experiments to which this discovery led, it was found that a portion of the distillate had condensed upon the cooler upper part of the still and had dropped back into the still where the temperature was sufficient to cause its disassociation into products of lower boiling point."

The Master: He is referring to the Newark instance?
The Witness: Yes, the Newark discovery.

"These and some similar observations soon led to the building of the stills with the upper part wholly uncovered

so that the desired partial condensation shall occur and the less valuable intermediate hydrocarbons be converted into kerosene in the manner described."

This, again, overlooks the fact that practically all of the cracking is really of the heavier constituents which never leave the still. The principal value of the uncovered still which they recommend is not in the fact that you might [fol. 848] crack a little bit of what drops back but that by uncovering the top of the still you necessarily make the distillation much slower than otherwise would take place, and therefore hold the heavy products in the still for a longer time. It is simply a question of the time they are held there, the dropping back being purely incidental.

Even John Laing who, for his time had as clear a conception as anyone of the mechanism of some of the important features of cracking, considered there was great virtue in this repeated condensation and dropping back into the still; in fact, he went so far as to design a process in which the vapors were taken off from one section of a still, condensed and cooled and dropped back into another section where they were revaporized into another condenser, repeating that operation three times, resu-ting in a tremendously wasteful consumption of heat and very little useful result taking place; this, as you see it, is merely the logical carrying out of this prior art theory of what the mechanism of cracking was.

Along the same lines I recall distinctly that around 1911 I was taught in my class in chemistry that the cracking reaction was absolutely due to the sudden heating of these drops of oil which were condensed on the top walls of the still and dropped back into the highly heated residue. That was supposedly a scientific classroom and that was the best explanation they had for this change which took place; in fact, it has been stated that the term "cracking" really originated—as a distortion of the term "cracking", referring to the noise in the still which happened when the oil dropped back in, and especially if there were any water condensed and dropped back into the still. I cannot vouch for the correctness of this statement, but it has been so stated.

The Master: You do not mean to say that they were oblivious to the molecular theory?

The Witness: Absolutely. It was apparently beyond their conception that cracking was a quite operation that was going on without any fuss or feathers in the main body of the oil in the still. It had to be due to some sudden change or some sudden heating or condensing and dropped back and the fact that it simply took place quietly in a body of oil apparently never was considered as a possibility.

The Master: Well, do you mean to say that they did not comprehend that the molecules of the original organic sub-
[fol. 849] stance were disrupted? and that there was a recombination of atoms?

The Witness: No, I do not mean to say that, your Honor. They understood that they were splitting apart the molecules; they understood that perfectly well, but they thought that the way in which those molecules were split apart was due to some one of these dropping back actions or something of that kind, rather than that it was something that was just proceeding quietly, solely as the result of the temperature to which the body of oil was heated.

Q. They thought it was the light part instead of the heavy part, did they not?

A. They thought the main part that was cracked was the part that dropped back into this heated oil.

Q. Was there any other theory current in the art, Mr. Wilson?

A. Yes.

The Master: Then, what they supposed happened in this Newark discovery did not happen; it was something else that happened?

The Witness: What they saw happening, that is, cracking taking place, did happen. Cracking did take place but the explanation which they gave for it was not the correct one. It was that explanation of dropping back which John Laing took and on the basis of which he built his sectional still and carried it to the logical extreme, if he carried it out at all, and then as a matter of fact it could not work.

The Master: Yes, but what they thought happened did not happen; it was not the particles that were dropping back that were cracked you say, but it was the body of the substance in the still maintained for a long time at a high temperature.

The Witness: Practically all of the cracking—of course, I would not say that some of this material that dropped

back did not crack slightly, but that is far from the main thing that is taking place.

There were also a few supporters of another theory: that even in this dropping back theory of cracking, that cracking without pressure was due mainly to a reaction which took place only in contact with very hot surfaces, particularly hot surfaces above the liquid level. That was, of course, somewhat closer to the truth and still not the truth. This is true of the latter part of the description by Gruen- [fol. 850] ing in "Exhibit 250," and also Aisinman in "Exhibit 118," near the middle of page 1486, describes Veith's theory as follows:

"The decomposition of mineral oil is based upon the contact of the same, or of its vapors, with the hot walls of the distilling apparatus. In this sense the cracking boiler kept a dull redness can be regarded as a large pipe where the heavy oils entered, decomposing by contact, thus resulting in a large percentage of aromatic hydrocarbons."

He further says, five lines further down:

"Thus, according to Veith, the apparatus should be so built and arranged that the heavy fractions and their vapors should be brought into contact with the overheated surfaces for as long a time as possible."

According to that theory the top of the still should be kept very hot to induce cracking, which came nearer to being correct than the other theory and still did not strike at the root of the matter.

Aisinman himself seems rather inclined to reject this idea, in favor of the older one. He recommended the cooled top of the still, because he goes on to say, page 1487, the second complete paragraph:

"Strange to say, the original idea which regarded the real value of the cracking process as a reduction of the temperature has been left unobserved and has been fully appreciated and considered only in more recent installations."

Again, John Laing, who, as I have mentioned, seemed to know more about what was really taking place in cracking than most anyone else of his time, in Exhibit 106, page

1376, put forth a modification of this hot surface theory to the effect that cracking is mainly caused by radiant heat from the walls above the oil surface which he said should be kept very hot.

I think that is all the citations I have and all of them go to show that they did not catch the idea in these prior art investigations that cracking was essentially a reaction which took place mainly in the main body of the liquid oil held at high temperature, and it is simp-y illustrative of the very many common misconceptions which were prevalent prior to 1912 and even beyond that time.

[fol. 851] The Master: I do not want to pass this before I get it clearly. How has this been demonstrated in modern research, that the cracking takes place in the main body of the liquid oil in the still?

The Witness: In a variety of different ways.

The Master: Does it rest merely on opinion?

The Witness: No. It is an absolute established fact.

The Master: How was it established?

The Witness: Well, you can take a body of oil in a bomb and immerse it under a lead bath, or in any other material, so that there is neither any part of the surface which is heated hotter than the main body of the oil, nor any part of the surface which is cold enough to drop back anything and you find that the reaction takes place at approximately the same rate as it does in these other stills. It is primarily a function of time and temperature which we now know for a fact in the cracking of oil. Of course, other things come in. When you want something,—some particular kind of product,—to the exclusion of other kinds of products.

The Master: Have these experiments been undertaken to demonstrate this?

The Witness: They have been undertaken in the course of amplifying our knowledge of what takes places in cracking, but they are theoretically——

Q. Some of them led up to the practical process like this patent you have just produced?

A. Yes and some of them led into investigations——

The Master: That patent might be clear opinion, you know. I am trying to find out whether it is a demonstrated fact or expert theory. That is what I am trying to distinguish between.

Q. Further on that subject, it has been found in practical work that the surface area, the fired area makes very little difference?

[fol. 852] A. Yes, although some of the earlier works thought that was the important thing in cracking.

Q. And the amount of dripping back makes very little difference as to cracking rate, and that has been demonstrated, has it not, providing you keep the temperature the same?

A. Yes. The important thing is the temperature of the main body of the oil in the still. That is in a bulk pressure distillation, or bulk distillation process without pressure.

Q. Now, this which you have referred to as the simple case of atmospheric cracking, have you found any disclosure either in the literature cited by the plaintiff or anywhere else of the true principles that underlie the process?

A. I have not found any clear disclosure of what it was that actually took place even in this very simple and common method of cracking.

When you come to the more complicated relationships of bulk pressure distillation and tube cracking, the general statements of the prior art go even further afield. Thus, again, even John Laing went so far as to imply in his patents and papers that it was possible to take a reduced crude oil in a four-foot diameter pressure still and make gasline from it,—make a commercial quantity of gasoline from it. Even with all we know today, it would be absolutely impossible to carry out any such operation, because before we have taken off more than a few quarts of material we would have a red hot bottom and have to interrupt the operation for the sake of safety.

The other similar extravagant claims and misstatements as to what was possible or what could be done or what had been done by inventors, according to their claim, led to many wide-spread popular misconception which persisted even until today as to what can and cannot be done in pressure distillation.

Thus, even plaintiff's expert indicated that it was quite possible to crack a reduced Mexican crude in a Burton still down to about a 30 or 40 per cent bottom, whereas anything even remotely approaching this would be absolutely impossible by reason of its cooking difficulties.

The Master: Have you had some personal experience that enables you to say that?

The Witness: I have, yes, sir.

[fol. 853] The Master: What was it?

The Witness: I tried to conduct operations on cracking reduced California crude just to see exactly what would happen, and even on a small scale still, which I shall show later is very much easier to operate than a large scale still, in handling dirty stocks. Before we got to 700 degrees the bottom became red hot and it was not possible to continue the operation with safety and to make more than two per cent of gasoline.

Similar experiments have been carried out by our company. Of course, these reduced crude oils are all very cheap and plentiful and everybody would like very much to crack them if they could, but as a matter of fact, none of defendant's processes even pretend to be able to handle reduced crude oil in bulk pressure distillation processes, even with all the improvements that we have made up to date.

There is another type of misstatement that is found in the prior art of tube cracking and bulk pressure distillation, which is illustrated by a patent to Potter, (U. S. Patents 43429.)

The Master: What is the date of the patent?

The Witness: July 5, 1864.

Mr. Rauber: Is that one of Petitioners' Exhibits?

The Witness: No, that is not one of Petitioner's Exhibits.

The Master: It is not an exhibit?

Mr. Rauber: No, not in evidence yet.

The Witness (continued) Who states clearly that his method of distilling coal oil or petroleum under pressure increases the quantity of illuminating oils and decreases the quantity of light oils, such as naphtha and benzine.

A similar and even stronger statement is made by Benton in plaintiff's exhibit 150. U. S. patent No. 345,564 of 1886.

Q. That is the one the plaintiff's expert said was a landmark in the art, wasn't it?

A. Yes, it was, I believe. Benton states clearly at several points in his specification that by the use of his process benzine present in crude oil to an extent as high as 12 per

cent is converted into a homogeneous high flash illuminating oil. Benton's disclosure, therefore certainly did not lead anyone to try to crack kerosene to make gasoline, since he indicated that he brings about a reaction in the reverse direction, that benzine, the light end of gasoline by being subjected to heat and pressure is converted into kerosene.

[fol. 854] The Master: How has that been demonstrated to be fallacious?

The Witness: By the very fact that we are, today, cracking kerosene to produce gasoline, and if you take a stream of kerosene, or gasoline, and put it through the cracking processes, having the two constituents in the same proportion that he did, you will actually tend to increase, rather than decrease the amount of gasoline present.

Of course Benton's reaction is the type that might be expected from theoretical grounds. According to the mass action theory, or law, at high pressure such as he mentions, it ought to tend to make more kerosene from gasoline, but for reasons which we pretty well understand, now, that theory is not applicable to the cracking reaction as we ordinarily carry it out, and the fact is that the reaction goes mainly in the other direction—that is, kerosene is cracked into gasoline, and not gasoline into kerosene.

In point of extravagant claims for his process, Noad takes the prize, in "Plaintiff's Exhibit No. 146." He states that his very simple type of cracking process requiring no pressure whatever. (Quoting from line 67, on page 1799—) "is of advantage in relation to known processes, in that it enables a much higher percentage, or practically the whole of the oil to be converted into volatile spirit, and avoids the production of more than a very low percentage of permanent gas and residual carbon."

If this statement were true certainly no one is going to the trouble of putting in an expensive, complicated and relatively dangerous, high pressure equipment, which they actually do in order to produce commercial cracking.

As a matter of fact, tube processes like those of Noad, are really much inferior to what he disparagingly refers to as the other known processes, because tube processes characteristically produce very large amounts of gas, and relatively low yields of gasoline or benzine, of a quality that is distinctly inferior to that which can be made by other types of processes.

The Master: Can you reinforce your opinion in that respect, by some definite citation of practice?

The Witness: Yes, sir. For example, by taking the Holmes-Manley process, which is one rather extensively used by at least two of the defendants, it is possible to take gas oil stock and with one passage through the system, to [fol. 855] make something like 40 per cent of gasoline, and only a matter of five per cent of gas and less than a tenth of one per cent, of coke, and then take the tar from that process and distill it in accordance with another patent, which will come into the case, and recover more charging stock, which can be put back in, until we eventually end up with a product of something like 80 per cent of gasoline, and not more than two-tenths of a per cent of coke, and probably something around ten per cent of gas.

I haven't the exact figures in mind at this moment.

Whereas, the very best I have seen in any type of tube processes—and all of us have extensively experimented with the tube cracking process— is to make—if we make in one pass, 25 per cent of gasoline, we consider ourselves very lucky not to make at least 12 per cent of gas. If we made 40 per cent of gasoline we would find at least 20 per cent of gas was produced along with it. The yield of gasoline would be very much less, instead of being more, as Noad claims.

Q. Mr. Wilson, yesterday when you were discussing the flash point on kerosene I asked you what was the percentage of vapor required in the atmosphere to produce the explosion or flash, and I think you gave the percentage by weight. Now, will you please give the percentage both [fol. 856] ways, weight and volume?

A. I have not seen the record but I think I probably misspoke myself. The concentration of kerosene vapor required to give a flash is approximately one per cent by volume, or approximately four per cent by weight in the air. That will vary somewhat depending on the kerosene.

The Master: You are speaking of kerosene or gasoline?

The Witness: Kerosene; the flash point of kerosene.

Q. You also, in discussing the volatility requirements of motor fuel yesterday, gave the reasons for the various requirements. Now, have you done some work on that personally, and under what circumstances?

A. Yes. While at the Massachusetts Institute of Technology I did quite a bit of work on the volatility of gasoline and published two or three papers, most of which were printed in the proceedings of the Society of Automotive Engineers and presented before that society and others,—at least one other—presented before the American Petroleum Institute.

The Master: What was the nature of these inquiries?

The Witness: To determine what the significant points on the distillation curve were to insure, first, the proper volatility of gasoline from the standpoint of distribution in the manifold, namely, the upper end of the curve; and, second, what was the proper point to specify to insure proper ease of starting in cold weather.

I think I was the first to point out the fundamentally different nature of the two volatility requirements for gasoline and to try to set up scientifically the proper points to specify on the distillation curves.

The common feeling was that the end point was the most important point, whereas my resu-ts showed that the 90 per cent was a much more important limitation than the end point.

The Master: And your observations were based on experimentations with gas engines in the laboratory?

The Witness: Not mainly with gas engines, but taking what we knew, namely, that we wanted to completely vaporize the fuel at a given temperature in the engine, we experimented to find what temperatures were required by different fuels to completely vaporize them, and compared that with the characteristics of the fuels and then also what temperatures were required to give certain concentrations of vapor [fol. 857] sufficient to be ignited and how they compared with the distillation curves of the fuels.

Since coming with the Indiana Company of course we have extended that to actual engine tests starting in refrigerated cold rooms and extended the work still further.

Q. Now, were there processes known in 1912 by which low-boiling hydrocarbons could be made from the heavier oils?

A. Yes, plaintiff's expert has cited quite a number of processes by which the low-boiling hydrocarbons could be made from heavier oils.

Q. Now, when the rapid expansion in the use of the motor car around 1910 or '12 increased the demand for and the price for gasoline why, in your opinion, were these processes not put into commercial use?

A. In my opinion, the answer is quite different for the different types of processes. For the ordinary bulk distillation without pressure, which was extensively practiced, the reason is fairly obvious, that it made mainly kerosene and gas oil with but very little gasoline, and there was not much they could do to change that condition, because the intermediate products of cracking distilled out of the still as fast as they were formed and it was not feasible to crack them to make any considerable yield of gasoline. So, while they would make low-boiling hydrocarbons, they were not the solution of the problem of making gasoline.

The prior art bulk pressure distillation processes were similarly disclosed and generally considered as being processes for the making of kerosene. And, furthermore on account of the very dirty stocks which they used, they were only suitable for a very light cracking to make kerosene, and there is not one of them which really could be operated commercially to do heavy cracking and make gasoline without getting a red hot bottom in a very short time and creating a hazard which would be out of the question in an ordinary refinery.

The tube cracking processes, which were the ones generally considered as being the proper way to make the lower-boiling hydrocarbons in the gasoline range, and which were considered to be and were of course much cheaper and safer to operate, were quite extensively tried out, but suffered from the drawbacks I have already mentioned. In the first place, the gas losses were high, the amount of [fol. 858] gasoline made per pass was usually very low, and particularly from the fact that the character of the product was so far different from that or ordinary gasoline and so difficult to refine to make anything that was light in color and of good odor, and free from gum-forming constituents that most of the refiners came to the conclusion that a cracked gasoline was bound to be a very inferior second grade product, not realizing that that was a characteristic primarily of the high temperature tube cracking processes, rather than of the cracking operation in gen-

eral. They tended to think that cracking was cracking regardless of the conditions under which it took place.

The fourth possible method of flash cracking was also undoubtedly considered but suffered from the same drawbacks of tremendous gas losses and a product which did not anywhere near resemble what they considered to be gasoline and which they did not know how to refine to make a satisfactory gasoline.

To summarize: In my opinion the reason the industry did not adopt any of these prior art cracking processes was because it was not possible by the use of any of them to make commercially useful gasoline, a marketable gasoline, at a profit. For that reason the refiners were all looking for some very marked improvement over any of the prior art processes in order to make it possible to thus produce marketable gasoline at a profit.

It is true that if the margin of cost between the heavier oils and the lighter oils had risen to a good deal higher point than it was in 1912, even these comparatively inefficient processes might very likely have come into use, but that was not the case for a good many years after 1912, and, of course, is not the case today.

I believe I forgot——

The Master: Were the rectification processes necessary to make the products of the tube pressure cracking operation conform to the required standard known in the laboratory at that time in 1912?

The Witness: It would probably have been possible by treating with the common re-agent of strong sulphuric acid and destroying anywhere from 20 to 40 per cent of the distillate to have left a residue which would be saturated and which would be fairly marketable as gasoline, but the tremendous consumption of acid and the very large re- [fol. 859] fining losses made that out of the question, so no one wanted to consider it.

The Master: Was the sulphuric acid process the only rectification process known to the laboratory?

The Witness: We ordinarily term it a refining process.

The Master: A refining process.

The Witness: No, there were a number of different processes, I believe in the literature but that is the only one that occurs to me that was known then by which it

would be possible to make a suitable product. I am not sure that I have in mind all of those that might have been available at that time but that is the only one I can think of.

Before we leave that question I think I neglected to mention this: the chemical or catalytic methods of cracking which were also known in a general way, as I recall it, before 1912, though I am not positive of the dates, but there the cost of chemicals made them again out of the question for making a gasoline at a profit, although the product was a good quality product. I may be wrong in the dates when that was first disclosed.

Q. What was the first gasoline-making process developed and operated on a commercial scale at a profit?

A. The Burton process developed by Dr. Burton and his associates of the Standard Oil Company of Indiana and first operated at Whiting, Indiana.

Q. I wish you would please describe the general features of this Burton process as conducted by defendant, the Standard Oil Company of Indiana in shell stills?

A. I have a diagrammatic drawing——

The Master: I have a faint recollection of shell stills but I did not quite catch the significance of that.

Mr. Wiles: We have two types of stills, what we call shell stills and tube stills.

The Master: Oh, yes.

Mr. Wiles: The shell stills are merely horizontal cylin- [fol. 860] ders, and the tube stills are like a tubular boiler. In the first place——

The Master: The term has been used once before but I could not visualize it. Go on.

A. If we can give this an exhibit number I can refer to it so it will appear in the record. I have here a diagrammatic drawing of a Burton shell still equipped with the radiator type of runback.

Mr. Wiles: The drawing is offered as Defendants' Exhibit 401.

(The said drawing so offered, was admitted and read in evidence, was marked "Defendants' Exhibit 401" and the same is herewith returned.)

A. (continued). The type of stills which are shown diagrammatically here are typical of those in our first row of 60 shell stills which are, except for the replacement of corroded parts and the installation of various minor improvements, the same as those first installed in 1912 and operated practically continuously since that time.

The Master: And now operated?

The Witness: During the last few months they have not been operated under temporary conditions, but operated until within a very few months. Probably if we tried to trace the history of these pieces many of them we would find to be a good deal like the jack-knife, the handle replaced and the blades replaced and other things, but they are essentially the same batteries of stills.

They consist of an 8 by 20 shell still with walls a half an inch thick, set over a fire box, either hand or stoker fired. I believe the first row is entirely hand fired, although a stoker is shown on this drawing; the still is connected by a pair of inclined vapor lines, such as are disclosed by Humphreys in his runback patents; to the reflux condenser of the radiator type as described by Moore in his radiator patent. The vapors, which escape condensation in this runback system and are composed mainly of gasoline, passing over into the water-cooled condenser and into the receiving drum, where the gas is separated while still under pressure and, the liquid passes out through a meter and a reducing valve. At the end of the operation, after the still is cooled down, the tar is withdrawn through the tar line shown.

[fol. 861] These stills are equipped with two sets of hinged false bottoms, such as described in a Humphreys patent which has not yet been cited, but is No. 1,122,002.

Mr. Wiles: That patent is offered as Defendants' Exhibit No. 402."

The said patent, marked "Exhibit No. 402," was admitted in evidence, and the same is herewith returned.

The vapor lines are twelve inches in diameter and, as I recall it, about forty feet long.

Q. How many are there of them?

A. Two of them for each still.

The Master: I suppose they are what I marked here as "Twin run-backs"?

The Witness: That is correct. Those are, of course, very much larger than the ordinary vapor lines on stills of this size.

The first thing which takes place in these stills, after cleaning and bolting on the end manhole cover, is to turn steam in through the top manhole cover, and fill the still with steam, displacing the air, in order to avoid any danger of an explosion when the oil is turned into the still. This is because, in any cracking operation of this type, hydrogen sulphide is formed, which attacks the metal of the still and forms ferrous sulphide. This ferrous sulphide tends to be pyrophoric,—that is, in contact with air it is likely to suddenly start glowing and form a source of ignition, and hence steam is always turned into the stills to prevent any danger of fire, and then the oil is run in. The charging stock is run in and the still filled about two-thirds full, allowing room for expansion as the oil heats up.

Q. Is that practice of steaming stills during charging, common in charging other stills?

A. Yes, it is quite generally used in charging coking stills, or in any still where there is any danger of fire from any such source.

Q. And that is simply in accordance with what was common practice long before the Burton process?

A. Yes, that is nothing new. The charging stock con-[fol. 862] sists, at present, of a mixture of virgin gas oil from crude, including the light ends from paraffine distillate, and the heavy ends from re-running the kerosene, plus naphtha bottoms, the heavy ends from re-running the pressure distillate, and cycle stock, which is the distillate from the pressure tar left from the previous cracking operation. That is what is known as a charging mix, the formula of which is kept standard.

I believe in 1912—at first, at least,—the stock did not include this cycle stock from the pressure tar.

Q. Is that cycle stock described in some patent?

A. Yes, that is described in the Burton patent, No. 1,105,-961.

Q. That is commonly referred to as the re-run patent?

A. Yes. I am not sure that that is the process, or the product patent. There is a process of re-running to make this cycle stock, and a patent on the new product.

The Master: That is the process patent.

Q. And the use of that is also described in another patent?

A. Yes, sir.

Q. What is the first step, after charging, in this Burton Process, as carried out in these shell stills?

A. After the top manhole cover is bolted on, the fire is started and the still is fired quite hard, heating the oil up.

The aim of this first, or dehydration part of the process, is to distill out the water which may have been accidentally present in the cycle stock, or which was left in by condensation of the steam. In order to distill out this water a certain amount of the lighter ends of the charging stock, which would be, probably, too light in character to be cracked anyway, are distilled out along with it, and collected in the drum—both the water and the oil. When the water stops coming over the dehydration step is considered to be at an end.

The Master: The water is drawn off from the bottom of the drum?

The Witness: Yes, sir.

The second step is what might be termed the digestion step, as now carried out. In this step the still is heated up, still at a rapid rate, but in order to prevent further distillation of the charging stock from the still, a pressure of about 20 pounds per square inch is maintained in the high line, which is shown connected to the receiving drum—the high line being the line which is now kept at a [fol. 863] pressure of 95 pounds per square inch, by gas made by other stills connected to the steam line. The stills in a battery are operated on an overlapping cycle, so that there are always some stills supplying gas to that high line, and, as I say, ordinarily, 20 pounds pressure is backed into the system, which puts a blanket of gas over the top of the still, to stop distillation.

The heating is then continued until at a temperature of about 700 degrees, but just before the pressure has reached 95 pounds, and before any running has commenced,

the fires are slackened off quite markedly, and for the last half hour, or hour, the rate of firing is slowed down to approximate that during the running period, which is considerably less rapid.

During this period there is quite a marked change takes place in the character of the charging stock—that is, there is enough cracking takes place in that hour, going from 700 to 725, to lower the initial boiling point of the stock in the still, from 50 to a hundred degrees, and to change the composition of the stock in the still, after which it starts off running at an approximately normal rate.

If the firing is continued at the full rate, until the still begins to come over, the temperature as they say, overshoots its mark—that is, it will rise to around 750 degrees, before distillation begins, and then cracking being quite rapid at that temperature, we distill off a considerable amount of a poorer quality distillate, and we have trouble settling the still back into its normal running rate, but by slowing up the fire for about half an hour, or an hour, and changing the character of the stock, the distillate begins at about 725° and we are able to start off in step, and then operate the running period more smoothly than would otherwise be the case.

The pressure during this digestion step, is rapidly approaching 95 pounds per square inch, and when it reaches 95 pounds per square inch, the drum is at once opened up into the high line, so that any excess gas can escape, without building up any higher pressure than that, and shortly after that the still begins to run, or come over, as the common saying is—the distillate showing up in the drum. That marks the beginning of the third, or running step.

[fol. 864] In some of our refineries the full 95 pounds pressure is backed into the still right at the beginning of the second step, and the still is simply left connected to the high line. That accomplishes very much the same purpose, and requires a little less attention, but suffers from the objection that the putting of such pressure on the still at that stage, tends to increase leaks of oil out around the rivets and seams, because every time you heat up and cool down a still, certain small leaks are started, and this is more serious in the earlier stages of the firing, because at cracking temperatures these leaks will quickly be plugged up by

the formation of coke in the tiny leaks, but those leaks do not plug up during the low temperature stage of the operation. So we consider it preferable to start from twenty pounds even though it requires more attention.

Q. And you so do it at Whiting?
A. Yes, sir.
Q. But Wood River does it the other way?
A. Yes, sir.

The Master: Where is Wood River?
Mr. Wiles: Wood River is just outside of Alton, Illinois, about forty miles from St. Louis.
The Witness: I think all our refineries except Wood River, back in about twenty pounds pressure at the start.

Q. How do these two methods of preventing distillation during the digestion step, compare with the disclosure of Dewar & Redwood, in this respect?
A. The operation is very closely analogous to that of Dewar & Redwood, who pump in gas to the stills, and prevent distillation, until the cracking temperature is reached, save for the fact that here we use gas already made by other stills, at the desired pressure, and hence it is more convenient than having to pump in air or other gas.

In these stills it requires from three to five hours to get the oil heated to the point that the water is all out, and [fol. 865] from five to eight hours, as a rule, to complete the digestion step, and to get the still in position for running.

After the still comes over the firing is so controlled as to make about the same amount as the ordinary crude still makes—that is, taking off three or four per cent. of the distillate per hour, and on each those stills the distillate generally runs around sixty per cent., or a little more of gasoline, the rest of the distillate, of course, also being useful in making kerosene, although by far the most of the kerosene that is formed, is returned to the still for further cracking.

The fact that the re-flux system is air cooled, of course means that it is somewhat subject to fluctuations in the amount of material that is condensed and returned to the still, and that tends to affect the quality of the distillate, but such variations are overcome in one of two ways. In

the first place, the radiators are generally surrounded by wind break apparatus, which can be opened and closed, to considerable extent, and thus vary the re-fluxing action; and, in the second place, just as in the ordinary still, if the distillate begins to come over a little heavier, the fires are slackened, or, if it is too light, the fires are speeded up, in order to make a heavier distillate, in the customary way used in operating refinery stills.

The stills start out cracking and running at about 725 degrees, and operate on a gradually increasing temperature, finishing at about 755 degrees, when something like 55 per cent. has been taken off.

That takes twelve to fifteen hours. The operation is stopped at this point, because although the stock, while originally charged to the still was a clean stock, which deposited very little coke, as the cracking proceeds, it gets dirtier and dirtier. This is due to the formation of these new heavy products that form along with the light products in this type of cracking operation, and it is found, by experience, that if you do not stop at about 55 per cent. off, you are apt to run into hot bottom trouble.

The policy of the refinery, is, of course, to stop the distillation before there is any serious danger of hot bottoms, because a hot bottom is a very serious and dangerous thing, and you cannot play with it.

[fol. 866] The Master: What do you mean by dirty?

The Witness: I should define that—although it is a common refinery term—I don't mean anything in the sense of any suspended matter in there. I simply mean that the stock, when cracked further, will produce a large amount of coke, which will settle down on the still. There is nothing about it that you can tell from looking at an oil—for example, this finished red oil is a highly refined oil, but is what we call a dirty stock for cracking—shown in bottle No. 13—although it looks very clean and has no dirt, properly speaking, in it—whereas, the gas oil——

The Master: Dirt means carbon?

The Witness: Dirty, in a cracking sense, means a stock which, if cracked to any considerable extent, will deposit appreciable qualities of carbon. Whereas, a gas oil, which looks like a dirty stock, and which has, probably, some suspended wax, or maybe a little actual dirt in it, is a clean

cracking stock because it does not tend to deposit much coke.

Q. I wish you would cover a little more in detail this progressive formation of carbon which you mentioned in your last answer.

A. To illustrate a little more clearly just why this is so serious, I ought to point out first that we are conducting a cracking operation at a temperature in the region where the strength of steel is decreasing very rapidly with increasing temperature. Up to 600 degrees the strength of ordinary steel is practically the same as it is at ordinary temperatures, but beginning at 600 degrees it drops off rather sharply, so that at 1,000 degrees its strength is only half of that at ordinary temperatures, and at 1200 degrees its strength is only a quarter of that. In other words, by the time you see any signs of a hot bottom on a still, the strength of the still has dropped to half its original strength or thereabouts, and if it eats up very much more you will be down to a place where your factor of safety is completely gone.

The Master: When you refer to strength of steel, do you mean tensile strength?

The Witness: Tensile strength or yield point. Possibly the yield point would be——

The Master: Bending point?

The Witness: The yield point is the point at which it begins to flow, and of course that means a distortion of the still, which is dangerous because it tends to thin out the [fol. 867] metal. The second thing is that as cracking proceeds, as I just mentioned, the stock tends to throw down more and more coke per unit of gasoline made. For example, between taking off 55 per cent of the gasoline distillate and 60 per cent of the gasoline distillate from these stocks we might throw down as much coke as would be formed in the entire operation up to 55 per cent. You get very little coke in the first twenty per cent of gasoline made from this kind of stock, and then you get a little more between 20 and 30, and more still between 30 and 40—a rapidly ascending curve. Unless you do stop before this coke begins to form too rapidly, you get a little more coke settled out at some point and a little more superheating,

which favors local cracking, and the coke builds up quite rapidly. That shuts off this area from the cooling action of the oil, as it is exposed to direct fire from beneath, it heats up rapidly once it starts.

That is the main job of the stillman, particularly toward the end of the running period, when he watches for any signs of a hot bottom and shuts down the still as soon as he sees any indication of it. And he can see it much quicker than you or I or any other person not trained in watching it. Furthermore, sometimes, and particularly if you are running a little too far,—taking off a little too much for a particular stock,—the stillman won't see any signs of a hot bottom, but there will have been just enough overheating to make a little bulge in the still, and if that is allowed to remain until the next run that will serve as a little pocket to collect carbon and is liable to give a much more serious hot bottom. So after each run, whether there is a sign of a hot bottom or not, the bottoms are carefully measured up to see if there are any signs of bulging, and if a bulge is found it is more than about an eighth of an inch deep, the bottom is heated up and pressed back into place with a special appliance that we have for that purpose, so that the next run will start out with an absolutely flat bottom and no chance for material to collect locally.

I might say it would not be possible, to take even 55 per cent of distillate off this kind of charging stock—of c-urse, the amount to be taken off will vary for different stocks—if it were not for the use of the Humphreys false bottom plates, which serve to collect the bulk of the carbon; but even with the aid of those, if you form more than a quarter of one per cent of coke in making the 55 per cent of distillate, you are very likely to go down with a hot bottom, and [fol. 868] if the refinery, after they change the stock in some way or change the operating schedule a little, begin to have more than a very small percentage of bulges or incipient hot bottoms, they immediately slow down their schedule, because the expense of pushing the bottom back into place and the risk of getting into the hot bottom troubles is so great that they would rather take off less and take the dirty stock out of the still.

Q. Do these objections to the formation of coke apply to all kinds of bulk distillation?

A. No, there are a great many cases of course, where those objections do not apply. If the size of the still is such that it can stand running red hot, or if the material of construction or the general arrangement is such that it can run red hot, then there is no reason to be particularly concerned about a hot bottom; also if you have the still immersed in a lead bath or some means of that kind to prevent it from being overheated, or, of course if you are operating at atmospheric pressure. In one of those cases is a hot bottom a serious or vital thing, but in a direct fired pressure still it is something quite different.

To a person not familiar with the cracking art, and particularly if he knows the practice in the coking art, the amount of coke required to make a hot bottom is generally a matter of surprise. I have a small sample here of coke taken from shell pressure still No. 8, which has shut down with a hot bottom. This coke had formed on the bottom, and made the hot spot, which, if the fires had not been shut down in a few minutes, would have resulted in the rupture of the still, because it builds up so rapidly. Even thinner layers of coke than that—and that is an average—frequently cause hot bottoms, and of course that is quite in contrast with the customary operation of the ordinary coking stills where you have a foot of coke and think nothing of it because of the atmospheric pressure.

The Master: From an eight to 3/16ths of an inch thick.

The Witness: Yes, from an eighth to 3/16th of an inch. I may say I had asked the foreman to let me know when they had a hot bottom, so I could get a sample, and it was three weeks before they had a hot bottom of any kind that necessitated stopping the run—showing how careful they are to avoid them in the operation of the stills, although a good stillman can see a hot bottom and if he is on his job he will get the still down before any damage results.

That is sample No. 18, and we might at this time finish the exhibit by offering sample No. 17, which is finished cracked gasoline refined by the Ellis process, which we won't describe for some time. That completes the list of exhibits.

[fol. 869] Q. How much coke is generally formed in one of these runs such as you have been describing?

A. Ordinarily about a tenth of a per cent of coke is formed when we are making, say, around 30 per cent of gasoline.

The Master: That is a tenth of a per cent by weight?

The Witness: A tenth of a per cent by weight. Of course, that varies, but if you get as much as a quarter of a per cent you are almost certain to get into trouble, even with the false bottoms.

As I indicated in coking mid-continent crude oil, even though you form only two or three per cent gasoline by cracking, the stock is so very much dirtier that you form about four per cent of coke.

Q. Just why is it that the coking operation can tolerate so much coke?

A. Because of the fact that it is at atmospheric pressure, and even though the still bottom is very seriously weakened, maybe to a very small fraction of its original strength, you have the same thickness of metal subjected only to the same weight of coke, which might be a pound per square inch compared with a pressure of 95 pounds to the square inch in a pressure still, so from that standpoint you have a hundred times the factor of safety that you have in a pressure still.

Q. What light did the ratio of coke to gasoline produced in the cracking operations commonly carried on in coking stills without pressure throw on the probable feasibility of bulk pressure distillation to make good yields of gasoline in stills of commercial size?

A. Certainly anyone familiar with the fact, as of course, all those skilled in the art were, that cracking to produce only two or three per cent of gasoline would make four per cent of coke and give a very bad hot bottom, which sagged from the mere weight of the coke, would not even want to consider such an operation under pressure, and that of course was the general attitude of the industry—that is, they would not want to consider it in a still of commercial size. Of course, it would be very simple in a very small scale still, where the strain on the metal would be nothing like as great. Anyone familiar with this situation would realize that some very fundamental and far-reaching improvement would have to be made before it would be feas-

ible to conduct a pressure cracking operation to make anything like 30 per cent of gasoline on a commercial scale.

Q. Now, what were the principal features which made it possible in the Burton process to make these yields of gasoline you have described on a commercial basis without coking trouble?

A. The proper selection of the cracking stock and the proper correlation of the temperature and pressure for that particular stock.

There are tremendous differences between the coke-forming tendency or dirtiness in a cracking sense, of different stocks. Gas oil of the intermediate boiling range disclosed by Burton, together with kerosene, are what are commonly known as clean cracking stocks because they make very little carbon for a given amount of cracking, as we now know; especially when the cracking is carried on by low temperature, bulk pressure distillation.

Q. You mean they are now known?

A. I meant to say they were now known, possibly I did not.

The next cleanest stocks are the lighter lubricating oils, but even so they must be mixed with a considerable amount of gas oil, as Chamberlain in his later patent mentioned, in order to make them clean enough to crack in a shell still. The heavier lubricants, such as cylinder stock, and reduced crude from practically any oil, form many, many times as much coke as these relatively clean cracking stocks. Whereas, reduced asphalt base crudes, like California or Mexican crude oils, are simply hopeless as a cracking stock. You cannot even begin to crack them before you get a red hot bottom in a direct fired pressure still. No one [fol. 870] that is familiar with the development of the cracking art since 1912 or with the publications and data on it would even consider trying to crack a heavy crude oil or reduced crude today or any time recently, in a bulk pressure still.

On the other hand, they were the logical stocks to try in 1912, because they were the stocks that had been cracked in the coking stills, and disclosed for cracking by bulk pressure distillation, carried out as a rule on a small scale, and also because they were so very cheap and very readily available.

I think these heavy reduced crudes like Mexican and California are almost always available as charging stock at considerably lower prices than gas oil, and available in tremendous quantities, and of course everyone has had his cupidity aroused, and everyone has been working to improve their processes to do away with the difficulties with coke. Still, with all the improvement which has taken place there is not one of the defendants' bulk pressure distillation processes that can even pretend to handle any reduced crude, let alone these heavier types of reduced crudes. They have been tried many times.

The Master: I would just like to interpose a question; I do not know any other way to satisfy my curiosity and increase my knowledge.

Going back to the period of 1912, were those who were practicing in the industry accustomed to selecting the stock, prepared stock for the operation of distillation, or were they all trying to operate on crude stock?

The Witness: Are you speaking of cracking?

The Master: Yes.

The Witness: The only cracking operation that was carried out in the industry on a commercial scale prior to 1912 was, of course, the coking operation which was always carried out on very dirty stocks, primarily the reduced crudes, or crudes. I may just add one thing——

The Master: Your previous answer there did not differentiate between the later knowledge and the knowledge of 1912. That is what I was trying to get a picture of if I could.

The Witness: As I will point out, the bulk pressure distillation processes, of which a great many were disclosed as being possible commercial processes for cracking, all disclose very dirty stocks. I will detail that later.

Q. How about such stocks as shale oil, Mr. Wilson?

A. As plaintiff's expert has said in his testimony, the [fol. 871] shale oils, or the cracking of shale oil, is quite different from the cracking of petroleum oils, in that in the case of shale oils the starting material is less saturated than the products produced, and since less saturation means that they have too much carbon for the amount of hydrogen, either you must add hydrogen to the oils to make saturated products or you must subtract carbon. As plain-

tiff's expert has said on page 664 of the testimony, "In cracking such oils, this excess carbon is liberated in considerable quantities."

Actually, from my experience in cracking shale oil or lignite oils, at least ten to twenty per cent of coke is produced if you carry out any considerable amount of cracking on them.

In other words, I quite agree with his statement that in this respect they behave very much like heavy reduced crudes.

Q. Now, in view of the facts that you have just stated, how do you account for the fact that the prior art bulk distillation processes such as those of Dewar & Redwood, Laing, Pielsticker, Barbet, Young and Reibeck, all of which operated indirectly fired receptacles are directed to the cracking of these very dirty types of stocks?

A. I think there are three principal reasons for that, or four.

In the first place, they were the cheap, available stocks which were not worth very much for other purposes, and therefore people were interested in making them into more valuable products.

In the second place, none of them operated on a commercial scale. They were operating in small laboratory or semi-commercial appliances, in which you could run at a red heat and have no particular difficulty, because even with considerable pressure the amount of stress on thick-walled retorts of small diameter, such as were employed, is not enough to make red hot bottoms a source of danger. It is only when you go to the very large stills that that element becomes serious.

They had, further, no appreciation of the fact of how rapidly coke troubles multiplied as you go up to larger sized stills. Some of them undoubtedly thought that their operations could be carried on with equal success on a very much larger scale, because they lost sight of the difference between large and small stills in their ability to tolerate coke, or the danger which resulted from running red hot.

The third, and in many ways, the most important reason is that they were mainly doing very light cracking to make [fol. 872] kerosene, and that is a very much simpler and very much easier problem from the standpoint of coke for-

mation than the heavy cracking that is required to make gasoline.

A few of them, to be sure, did run clear down to coke in their small pressure stills and got red hot in very much the same way that a large atmospheric coking still gets red hot, and in such cases they did make a little more gasoline than the ordinary operations where they were running for kerosene.

The Master: Did anyone of the disclosures of Dewar & Redwood, Pielsticker and Laing result in their adoption commercially?

The Witness: No, none of them were used commercially to any extent, as for as I know.

I want to refer to one example of this running down to coke which Krey describes in Plaintiff's Exhibit 247, where he speaks of operating "on a large scale" here and in his patent, but in disclosing his experiments in detail in this exhibit, it appears that his still had a capacity of only 500 kilograms, or about 150 gallons, which is, as you see, about 2 per cent of the size of a Burton still, which holds over eight thousand gallons.

Of course, coking operations under pressure are perfectly possible in stills of this small size, if the walls are thick enough in the retort. He states definitely that his fired surfaces got red hot. He was apparently quite willing to tolerate that and could tolerate it in his still because even with that comparatively small apparatus, two per cent of the size of Burton's, his wall thicknesses were three times that of Burton's. In fact they were a little over, an inch and a half thick, whereas Burton's apparatus has walls only half an inch thick. If you were to build a Burton still walls with thicknesses corresponding to those of Krey's so as to get equal ability to run red hot you would have to build stills with walls over a foot thick, which of course, would be out of the question.

Yet, even with Krey's thick walls he admits in this exhibit 247 that he had two still failures and he finally settled down to an operation much like most of the other prior art, which was a light cracking to make mostly illuminating oil, with only six per cent of benzine, as indicated on page 4229.

So, while some of them did experiment with heavier crack-

ing, even on a small scale they had so much grief they generally came back to the very light cracking.

[fol. 873] Those, I think, are the principal reasons why the prior art characteristically worked with very dirty stocks.

The Master: Is it your position that giving Dewar & Redwood, Pielsticker and Laing, the utmost credit for all that they discovered and communicated to the world, it required still further discoveries in order to make them available for commercial use in the cracking of gasoline?

The Witness: Still further discoveries and very radical improvements to make commercial quantities of gasoline.

Q. I wish you would now please compare in some detail the coking troubles in cracking by bulk distillation to make kerosene, and cracking the same stock to make motor gasoline?

A. In cracking to make kerosene, of course, all you want is an increased yield of distillate of proper viscosity for burning in lamps, and as I pointed out, a comparatively small amount of light material in the lower end of the kerosene, enables you to add a similar or greater amount of material boiling slightly above the ordinary kerosene range. So that if, for example, you conducted a very light cracking which tended to destroy some of the heavy ends and made, say, ten per cent of light ends of kerosene, since there is available in almost any crude oil a great deal of material just a little too heavy to go into kerosene by itself, you can by this means of adding a small amount of light ends, take on possibly twenty or thirty per cent of heavy ends and add to the kerosene cut. So that the amount of cracking that is required to make a thirty per cent or forty per cent increase in the yield of kerosene from a crude oil, and thereby be very profitable from the kerosene standpoint, is really very small and probably only involved the production of two or three or four per cent of gasoline, or something on that order.

A commercial cracking process to make gasoline would of course have to make five or ten times as much gasoline as was necessary for a light cracking to markedly improve the yield of kerosene, and, as I pointed out, the coke formation at the first of the cracking operation is always much

less than it is towards the end, so that it is probable that to make five to ten times as much gasoline, would involve the production of twenty to one hundred times as much coke.

Q. Now, please state how and why the size of a directly fired still affects the coke or hot bottom troubles?

A. It is a perfectly simple matter in the laboratory to operate a six or eight inch pipe with walls a half an inch [fol. 874] thick under substantial pressure even at a red heat. We have done it many times, just as the prior art did it. In fact, we have done it in trying out some of these prior art experiments. The fundamental reason, of course, is that in a small container, say six inches in diameter, with walls a half an inch thick, the strain on the metal is, of course, determined by the area on which the pressure acts, and therefore the force tending to pull apart the two halves of a six inch diameter container is relatively small; but if we increase that container to eight feet in diameter, with that enormously increased area over which the pressure acts, to get equal strength we would have to use walls a fo-t thick, which, of course, would be entirely out of the question.

That is the first important difference between small scale and large scale stills.

The Master: It is a matter of relativity?

The Witness: A matter of relativity.

A. (continued). The second thing which tends to increase coke troubles in a large scale still is the fact that as your diameter increases, the depth of oil which is cracking and producing coke increases, and therefore the amount of coke which settles down on a given square foot of the bottom increases in direct proportion to the diameter.

The third thing which tends to make coke troubles more serious, is the fact that the amount of heat that you must apply to any square foot of the bottom is, in general, roughly proportional to the height of the oil which has to be heated up; and, of course, forcing in the heat more rapidly means that the development of a hot spot is more serious and the rise of temperature of the still wall when it is insulated by a little layer of coke, is very rapid.

Without attaching too much significance to the actual figures we might say then that in comparing an operation in a still one foot in diameter with one five feet in diameter, that the general coke difficulties would be five times five, times five, times as great as in the small still, or one hundred and twenty-five times as great.

As I say, you cannot measure coke difficulties in strict numerical fashion, but we have something like a five-fold factor due to greater strain on the metal with increase of diameter; we have something like a five-fold factor due to more coke which settles down, and something like a five-fold factor due to the fact that five times as much heat must be forced in to keep the greater body of oil cracking [fol. 875] and distilling off its products.

In any case there is simply no comparison between the coke difficulties on a small scale and on a large scale.

The mere fact that a stock operates satisfactorily without blowing up a still on a small scale throws absolutely no light on the question of what you can do with on a larger scale. On the other hand, with the experience that we have acquired since 1912 by a lot of investigation, we do know about what per cent of coke we can tolerate in a large still. That has got to be acquired by experiments on a large still, but having that information available we can then conduct a cracking operation in a moderate sized still, being careful to preserve the same temperatures and cracking rates and scrape out the coke from the end of the run and weigh it, and if we find that it is around a tenth of a per cent, we will know we can carry out such an operation, in all probability, on a large scale.

Whereas, if it is much higher than that we know that such an operation would not be successful in a large scale. That can only come, of course, by having experience with both the large and small scale apparatus.

Q. That experience you have acquired only since 1912?

A. Yes.

Q. Now, I wish you would compare, from the standpoint of coke and bottom troubles, an operation such as the Riebeck operations, which are stated in his patent to be on a large scale, and are shown in Plaintiff's Exhibit 246, running so as to make the yield of products six per cent of gasoline, 0.75 specific gravity, (57° Baume), and 33 per

cent illuminating oil, with an operation conducted on the scale of the Burton operations, making upwards of thirty per cent motor gasoline.

Mr. Rauber: I do not think the witness has yet stated that he has worked this Riebeck process. Has he?

The Master: No, he is an expert here and can give his opinion whether he has worked it or not.

Q. Go ahead.

A. A still such as Krey's to hold 500 kilograms, or 150 gallons, might suitably be made in the same relative proportions as Burton's still. We will assume for the purpose [fol. 876] of discussion, that it was a still two feet in diameter and seven and a half feet long. That is a quarter of the ordinary dimensions of the Burton still, which means that the capacity of the still is 1/64th as great, varying with the cube of diameter.

From the fact alone, we may say in a rough sort of way, that Krey's ability to handle coke would be one-fourth times one-fourth times one-fourth, or one sixty-fourth as great as Burton's because of the smaller diameter, which means smaller strain on the metal, less coke per the unit surface, and less heat put in per the unit surface. In addition to that, Krey's stills had walls three times as thick as Burton's——

The Master: You are talking about Krey but he has asked you about Riebeck. Are the two the same thing?

The Witness: The two are the same thing. Krey is the man who developed and did all the experimental work on the Riebeck process. The patent was assigned to the Riebeck Montanwerke and that is why it got the name of the Riebeck patent. I am speaking of his operation in Exhibit 247.

The Master: Pardon the interruption.

A. (continued). I just stated that Krey's walls were three times as thick as those of Burton, so that he had another marked increase in his ability to handle a given amount of coke, and in addition to that, he only made about a fifth as much gasoline, which as I have shown, means a very much smaller amount of coke because of the lighter cracking.

As a result of all of that, I should say that Krey could handle successfully in his type of apparatus a stock which was at least one hundred times as dirty, that is, tending to form one hundred times as much coke, as one which Burton could handle in his type of still.

Q. In spite of this advantage and ability to tolerate coke, did Krey have hot bottom troubles?

A. Yes, he did. He speaks of running red hot, and when he tried to carry his cracking a little further than this operation to make six per cent. of gasoline, he had two split bottoms.

Q. Now, knowing the result of this Krey, or Riebeck operation, would one expect to be able to handle such stocks for the manufacture of commercial quantities of gasoline?

A. No, any practical refiner would shy away from such an operation, without any question, if he was asked to conduct it on any considerable scale.

[fol. 877] Mr. Royce: What stock is meant by "such stocks"?

Mr. Wiles: Such stocks as Krey describes.

The Master: That is, stocks a hundred times as dirty as the stocks handled by Burton.

Mr. Royce: That is not what Krey says.

The Master: That is what the witness says.

Q. Does the character of the tar produced, by any of the prior art, pressure distillation operations, throw further light on the severity of the cracking which was contemplated?

—. Yes, it does. The very frequent reference to the suitability of the bottoms, from a cracking operation, for making lubricants, are clear evidence of the very light cracking which they contemplated.

Those references are very frequent—for example, in "Plaintiff's Exhibit No. 247," 4225 and 4226, and in "Plaintiff's Exhibit No. 97," page 1289 to page 1291, and also in Potter's patent, U. S. No. 43,429, which I mentioned yesterday—there is a similar disclosure of the use of the bottoms from the cracking, for making lubricants, and Barbet, in "Plaintiff's Exhibit No. 136," even indicates that his residue can be pressed commercially, to make paraffin wax. That is on page 1692.

Such bottom stocks containing valuable lubricants, and being capable of being pressed for wax, are obviously enormously different from the black, unpressable, heavy, gravity, low viscosity pressure tar, which Burton produced, and describes in some of his patents. There is simply no comparison between the character of the products, and if you carry out heavy cracking to make gasoline, the bottoms are absolutely hopeless for such purposes; whereas, it is quite possible that with very light cracking, to make kerosene, we could make a good quality of lubricant from the tar.

The Master: What does that demonstrate?

The Witness: That is a further indication of the light cracking, which was contemplated in the prior art, and which was all that was necessary to make kerosene.

The Master: So that it demonstrates that they did not carry their processes—and were unable to carry their processes, commercially, as far, or to as great an extent of reduction, as by the later processes?

The Witness: That would not of itself, demonstrate that they were unable to carry it further. It would demonstrate that they did not carry it further, and would explain the fact that they were able to handle such dirty stocks and get away with it, because of their light cracking in small scale apparatus, which stocks we know are hopeless, and did not meet the problem in 1912.

The Master: But they then had a commercial reason for not carrying it so far, and it was not profitable to carry it so far?

The Witness: Yes, sir, they were mainly interested in cracking for kerosene, and very light cracking in small stills, to make kerosene, could be carried out with dirty stocks, but that would not be any indication as to what would be possible in making gasoline.

Mr. Wiles: The Potter patent, which Mr. Wilson produced yesterday, I wish, now, to offer in evidence as "Exhibit No. 403."

The said patent, so offered, was admitted in evidence, was marked "Exhibit No. 403," and the same is herewith returned.

A. Burton's fundamental contribution to the art of cracking was the disclosure of a direct fired pressure crack-

ing process, starting with an available stock, and capable of being operated on a large scale, for the production of [fol. 878] commercial quantities of gasoline, and yet maintain a black bottom to his stills.

The Master: That was his contribution by the patent referred to, No. 1105961?

The Witness: Mainly by that patent—oh, no—patent No. 1049667—the original Burton patent. Of course later patents followed and brought further, commercial success——

The Master: That was the process patent?

The Witness: Yes.

Q. Will you summarize, briefly, just what expedients the prior art did employ in order to avoid hot bottom difficulties?

A. Of course the simplest thing they did, and which was the most broadly employed, was to avoid the use of pressure, in which case hot bottoms did not give any particular difficulty.

The second thing they did was to work on a small scale, in case they did want to operate under pressure, which, of course, was not a solution of the hot bottom difficulty, but it merely gave them an ability to tolerate it, but it did not give them a process which could be operated on a commercial scale.

A third type of expedient, which really included a great many different expedients, was to avoid the use of direct fire. If, in some way, you can keep the flame from playing on the bottom of the stills, you would, of course, lose in the heat transferring capacity, but you would make it very much easier to prevent overheating of bottoms, even if you got coke on the bottoms.

Expedients of that type include surrounding the still with a thick layer of refractory, such as Dewar & Redwood advocate, the use of lead baths to surround small stills, the direct injection of molten metal, such as lead, into the stills, and, in a broad sense, the use of internal electrical heaters.

In all these cases you automatically prevent the still walls from rising very much above the temperature of the oil, and, hence, avoid hot bottom difficulties, but none of these expedients have been found practicable, or economical in use on a commercial scale.

The Master: Mr. Wilson, the contribution that you referred to, of Burton to the art, seems to be somewhat different from that which his patent that you referred to a minute ago—I am looking at the wrong patent, though—from what his patent emphasizes as being his contribution. And, therefore, these real contributions which you refer [fol. 879] to, are inferrable from what he evidently was trying to disclose. In other words, he is referring to the residues, in his patent?

The Witness: No, your Honor. There is some legal language mixed up in that patent, which tends to obscure what Burton really had in mind. But it is quite clear on studying—and I will be glad to bring that out—that he is talking about the distillate—he is not talking about the residuum,—that is the refinery term, but he is talking about what is left over of the distilled products of petroleum,—which his lawyer, apparently, called the residue—after you have made the maximum yields of gasoline, kerosene, and lubricants, and wax,—which is the gas oil cut. The gas oil cut is what is left over, in a refinery operating on the cooking process, after you have made the maximum yields of gasoline, kerosene, lubricants, and wax.

The Master: What does he mean by this:

"My invention relates to an improvement in the treatment of the high boiling point residual portion, and particularly the residue of the distillation of the paraffine group, or series of petroleum, after the low boiling point distillates have been removed, to obtain a lighter boiling point product, of the same group, or series"?

The Witness: You have to read the whole patent to be perfectly clear as to what material he was disclossing, and there are several points which bring that out quite clearly.

Down in the next paragraph he speaks about naphtha, the boiling points of which range from 75 to 350 Fahrenheit, and the illuminating oils that range from about 350 to about 600, and also the gas oils, with boiling points ranging from about 600° F. to 700° F.

That shows clearly that he is talking about this intermediate product of distillation, which is not a reduced crude. It has this intermediate boiling point of 600 to 700 degrees Fahrenheit, which clearly identifies it as a product

of distillation. Of course the reduced crudes have a boiling range around 700 to 1100 or 1200 degrees Fahrenheit.

Q. In view of the Master's question, at this time I want to hook this up a little closer to the testimony that you gave yesterday.

There are just three processes that were in common use, and that all refiners knew about in 1912, for the running of crude oil—isn't that correct?

A. Yes, the three that I described.

[fol. 880] The skimming process produced three products, gasoline, kerosene, and reduced crude. No lubricants and wax are produced in that method of running crude.

Q. Is there some antecedent process that the crude oil is subjected to before Burton starts with it?

A. He is clearly referring to the coking process or——

The Master: What you meant by that question was whether at the time——

Mr. Wiles: Yes.

The Master: Some antecedent steps had been taken upon the material?

Mr. Wiles: Sure.

The Master: The crude.

Mr. Wiles: That Burton speaks about.

The Master: Which he assumes as a starting point.

Mr. Wiles: Yes.

The Witness: Yes.

Q. What antecedent process or processes is or are referred to as having previously taken place?

The Master: They are set forth in the patent.

A. They are processes which make lubricants and wax as well as gasoline and kerosene, which means either running for steam refined, where your residuum becomes a cylinder stock, or running by the coking method of operation, where you have no residuum. Those are the only two methods which produce lubricants and wax.

Q. In those processes the only two which you just stated could be referred to, what is there left over after you have taken gasoline, kerosene, lubricants and wax?

A. The gas oil, boiling mainly between 600 and 700 degrees Fahrenheit.

Q. And that is all?

A. The slop cut, as I pointed out, is generally re-run with further crude.

The Master: Then it is your opinion that construing the language of Burton's patent No. 1049667, that the material he was referring to to be dealt with in that process patent was gas oil?

The Witness: Yes, sir.

Q. I do not know whether you finished the answer to that question about the prior art expedients.

A. I think I had not finished that answer. In addition to these expedients there are some prior art process- which could be operated with a black bottom while they were doing very light cracking on a small scale to make kerosene, but could not have been pushed to the point of making anything like commercial quantities of gasoline without serious hot bottom difficulties, even on a small scale. Probably Dewar & Redwood could have run somewhat longer than a good many of the others because of the fact that their retorts were, as I said, protected by a refractory from direct contact with the flame, but on commercial stills that would not be feasible and would greatly interfere with the rate of heat transfer into the still.

Q. What were the principal factors in Burton's success in developing this kind of process?

A. I should say that the principal factors in his success in developing this type of process was that he first selected a type of process, namely, bulk pressure distillation, at moderate temperature, which he found gave a better yield of a more satisfactory gasoline than the tube cracking processes or other types of processes which were available in the prior art, and then he discovered a combination of stock and temperature and pressure for cracking that stock, which would give him a high yield of gasoline and yet maintain a black bottom on commercial stills. The use of pressure condensation — his process also increased the quantity and improved the quality of the distillate which he obtained.

Q. What bulk pressure distillation processes prior to 1912 are described in the 146 references cited by the plaintiff?

Mr. Rauber: I object to the question, drawing a wrong inference there, and not being true. We did not cite that many references.

Mr. Wiles: That is the way I counted them. I will say "numerous references."

[fol. 881] A. There are twenty-eight references prior to 1912 which describe or bear upon bulk pressure distillation. However, many of these references describe essentially the same process, so for purposes of simplification I will consider the processes rather than the exhibits. There are really only seven distinct bulk pressure distillation processes described in these twenty-eight references, as follows:—and I will give all of the twenty-eight references in mentioning these seven types of processes:

First is the Young process described in Exhibit 93; second, the Dewar & Redwood process described in Exhibits 103 and 107, and also in a British patent to which I shall refer later; third, the Laing process described, with various modifications, in Exhibits 104, 105, 106 and 230; fourth, the Pielsticker process described in Exhibits 133 and 134; fifth, the original Barbet process described in Exhibits 136 and 141; sixth, the Riebeck process or Krey process described in the basic patent Exhibit 97. The application of this process to various charging stocks and studies on the character of the products are described in a variety of publications Exhibits 98, 99, 100, 101, 102, 152, 153, 246, 247, 248 and 250. And, seventh, the laboratory glass tube experiments of Thorpe and Young, Lobry de Bruyn and Jacunski, which are described in references 94, 95, 96, 151 and 249. Thorpe and Young also repeated their experiment in mercury bottles. None of this group was meant to disclose practical cracking processes, but they are properly a part of this bulk pressure distillation art.

Q. Do you find in any of these prior art bulk pressure distillation processes any such disclosure of stock, temperature and pressure condensation as would produce results similar to those obtained by Burton?

A. No, I do not.

Q. Do you find in any of these processes any disclosure of stock temperature and pressure capable of producing such a result, even if you add pressure condensation to their disclosures?

A. No, I do not.

In spite of the fact that, as I have pointed out already, the careful correlation of pressure and temperature is of absolutely fundamental importance to the carrying out of an operation of bulk pressure distillation for the produc-[fol. 882] tion of any particular product, there is no disclosure of operating temperatures and pressures for any of these prior bulk pressure distillation processes, at least so far as I have been able to find. In fact the almost complete absence of data as to the operating temperature of the oil is so striking that the jest of plaintiff's expert, to the effect that Burton must have patented a thermometer pocket on a pressure still really serves to bring out an important point in Burton's invention, that he was the first to disclose the proper operating temperature and pressure for any stock to make any desired product in a bulk pressure distillation process.

Q. Were these prior art bulk pressure distillation processes capable of being operated with a black bottom to do any cracking?

A. Yes, as I have indicated, several of them could do light cracking and make a considerably increased amount of kerosene on a small scale, and yet maintain a black bottom. Probably Dewar & Redwood could have run the longest of any of those cited.

Q. Did any of these processes disclose cracking and available stock which was clean enough to produce commercial yields of gasoline in direct fired stills of commercial size, without developing hot bottoms, even if they had had the benefit of Burton's disclosures of temperatures and pressures?

A. No, there are no such disclosures available stocks which were clean enough to accomplish Burton's result. Some of the purely laboratory processes disclosed stocks with which I am not familiar and which may or may not have been clean, but none of them were available at a price which would have made it at all interesting to crack them into gasoline. I will mention them specifically later. They are all high priced materials, and there could have been no profit in cracking them to make gasoline, even if they had been clean.

The Matser: The demand for an increasing supply of gasoline you say began in 1912?

The Witness: No, for a number of years before that, the refiners realized that they had to get busy in some way to increase the yield of gasoline. I think that is an historical fact that is pretty well established, and a great many were working on the problem in the peirod from 1908 on.

The Master: Assuming that the demand became more acute in 1912, taking that for the starting point, do you know of any commercial operation in the United States or [fol. 883] any place else which employed any one of these prior art processes to produce gasoline?

The Witness: No. I feel quite sure they were not employed commercially. They were undoubtedly tried out by people who were trying to find some way of making gasoline, but they were never put into commercial use and could not have been put into commercial use on a larger scale. The prior art processes which purported in any way to be commercial, all disclosed very dirty stocks, entirely too dirty to accomplish Burton's result.

Q. Was it well known prior to 1912 that gas oil would crack under any conditions, and, if so, what bearing does this have on its use in bulk pressure distillation?

A. Yes, it was of course known that gas oil would crack; in fact its very name implies its very essential use for cracking in gas-making. However, the processes used commercially for that purpose were all these very high temperature tube or flash cracking processes, the product of which did not resemble the commercial gasoline then available, and the losses in gas and other difficulties did not make them at all attractive to any one who actually tried them out. Burton shows in his file wrapper that he did try tube cracking and was forced to discard it. The commercial processes as a rule that were used in cracking gas oil only made from fifteen to twenty per cent of liquid products of a type quite different from commercial gasoline. In any case none of this cracking of gas oil to make gas indicated in any way that gas oil could be cracked at the temperatures and pressures which were reasonably obtainable in bulk pressure distillation, such as Burton employed, or that it would permit, if cracked in direct-fired bulk pressure stills, the making of a commercial yield of gasoline, or that the product obtained would be a marketable gasoline. It was simply part of a different art, and

what we learned from that art did not indicate anything as to the suitability of the stock or the character of the products, if any, which would be produced under Burton's conditions.

The Master: When you say a different art, you mean the gas art?

The Witness: The high temperature flash or tube cracking art.

The Master: Which was the gas art?

[fol. 884] The Witness: Yes, which was commercially used for the production of gas.

The Master: Illuminating gas?

The Witness: Yes, or for the enrichment of illuminating gas.

Q. Returning to these prior art bulk pressure distillation processes cited by the plaintiff, most of which you said could probably start cracking with a black bottom, hence could maintain a black bottom if their stocks had been clean enough, what stock or stocks come closest to Burton's gas oil boiling mainly between six and seven hundred Fahrenheit?

A. Probably the nearest thing disclosed in these prior art references is the liquid paraffin and olefine, boiling between 252 and 255 centigrade—that is 486 to 491 Fahrenheit—which is disclosed by Thorpe and Young in Plaintiff's Exhibit No. 96, particularly on page 1267. They separated this low boiling out from the original cracked distillate, which they made by cracking paraffin in their glass tube or mercury bottle experiments, and they redistilled it under pressure in their glass tubes. However, on comparing this product with Burton's gas oil, it is clear that, first, its low boiling range—that is, 486 to 491, as compared with 600 to 700, which is the bulk of Burton's—and its high unsaturation, which is brought out here, and the fact that it itself is a product of cracking, all clearly distinguish it from Burton's highly saturated gas oil from crude, boiling, mainly between six hundred and seven hundred degrees Fahrenheit. However, regardless of these distinctions between this stock of Thorpe and Young's and Burton's, which are very real, Thorpe and Young clearly state in the middle paragraph on the page that this material:

"May be repeatedly distilled backwards and forwards in a sealed V tube without suffering the slighest change." They conclude their discussion of that experiment by stating,

"A further proof of its unalterability on repeated distillation under pressure is afforded by its reaction with bromine." and they refer to tests made on the product before and after, which show that it did suffer no change. This is clearly not a disclosure, then, of cracking anything. It is a disclosure that this stock and similar ones cannot be cracked by bulk pressure distillation, at least under the conditions that Thorpe and Young used.

The Master: It arrived at a stable product?

[fol. 885] The Witness: It arrived at a product which they were not able to crack further.

A. (continued). It is also true that in deciding what stocks came closest to Burton, that some of the disclosures as to cracking are so broad as to include almost anything.

Thus, De Souchs, Plaintiff's Exhibit 156, says near the bottom of page 1852, and I may add that I have corrected this translation in certain minor respects:—I am reading from the last paragraph on page 1852:

"The invention (that is, a certain cracking process which he disclosed) is applicable in any case where a rapid and easily regulable production of gas vapors or liquids is desired for stationary or automobile motors, for lighting, for distillation, disinfection, perfuming, and, in general in every industry where gas vapors or liquids are produced. It is suited to all kinds of solid, liquor or powdered substances capable of generating gas vapors or liquids."

Of course, any disclosure as vague as this, covering almost everything as a charging stock, is meaningless as a reference to Burton's specific disclosure of a stock which is suitable for bulk pressure distillation to make commercial yields of gasoline and yet maintain a black bottom.

It is also true, of course, that some of the processes disclosed in the prior art started with reduced crude which contain, as a part of their constituents, gas oil, but the whole virtue of Burton's disclosure is the use of the gas oil without the crude and the mere presence of some material in this

boiling range as part of a very dirty stock is, again, meaningless as a reference to Burton's disclosure.

Q. Plaintiff's expert said on pages 514 to 516 of the record that Crew disclosed the cracking of gas oil. What are the facts with regard to this reference?

A. This reference which plaintiff's expert quotes is similar to those just referred to, in which gas oil is re-run along with crude and so could not give a black bottom process, whether or not the gas oil was cracked.

Furthermore, the process is at atmospheric pressure, which means if the stock were clean, it would lack the utility that was present in the Burton process, since atmospheric pressure processes can operate quite satisfactorily, and do every day, on very dirty stocks.

And, furthermore, the fact that it was at atmospheric [fol. 886] pressure meant that practically all of the gas oil, even if it was put back into crude would be redistilled without cracking.

Furthermore, there is no indication that Crew did or could or wanted to crack his gas oil. His object, as he sets forth quite clearly, was to re-run a little off-color kerosene or gas oil produced in the first running of crude, along with an additional lot of crude, hoping by that means to improve its color and get a better colored product. And, while gas oil would be distilled out without appreciable cracking in any such process it is quite true that the re-distillation would improve its color. That is a means very frequent- employed—in fact, most generally—to improve the color of a distillate—the coloring matter tending to be left behind in the bottoms.

This reference of Dr. McKee, which he cited and read on page 515 is also interesting in its description of the residium in the cooking stills, which he used for this cracking operation. I quote from Dr. McKee's quotation in the middle of Page 515:

"These are generally buit on the cylinder pattern and should not much exceed two hundred barrels capacity, and are set in banks of three. In stills of larger capacity it has been found in practice, that the quantity of solid cake residue is so great that the heat necessary to drive the still to dryness is so intense as to severely strain the bottom making frequent repairs necessary."

It must be remembered, of course, that Crew is talking about ordinary atmospheric distillation, where we have only the weight of the coke to ditsort the still. Certainly no one would care to consider operating such a process under pressure with that type of still, and it also proves very clearly that Crew was a long, long way off from having a clean stock, even though he did run a little gas oil along with his crude oil.

Q. I note that in plaintiff's exhibit 247 the article of Dr. Krey on the Riebeck porcess, the charging stock is referred to in some parts of the translation as heavy gas oil. Please state how the material there referred to by that name compares with the petroleum distillate boiling mainly from six hundred to seven hundred which Burton discloses as his charging stock?

A. A little examination of this exhibit shows that Krey's heavy gas oil is entirely different from Burton's petroleum distillate, boiling mainly from six hundred to seven hundred [fol. 887] degrees. Thus, in the first paragraph of plaintiff's exhibit 247, he refers to his oil as a heavy paraffin oil, which is generally used for gas manufacture, (which is, of course, where it gets its name, "gas oil",) but sometimes is used to make lubricants which indicates that it is much heavier than Burton's gas oil, which of course contains no lubricating stock.

This is further borne out by the statement on page 4228 where he gives its specific gravity as 0.9 or 25.7° Baumé, which is right in the middle of the lubricating oil range; whereas Burton's gas oil has a gravity of around 32.

It also appears from this page that when only fifty to sixty per cent of cracked distillate is taken off, the residue in the still is "a thick tar-like oil, specifically heavier than water, and with eighty per cent there remains coke" the last referring to taking off eighty per cent distillate. The low gravity and the extreme dirtiness of the stock, (that is, the fact that they only take off eighty per cent of it and left a residue of coke) of course shows it was a very dirty stock. The low gravity and extreme dirtiness of the stock appear to be further explained on pages 4225 and 4226, where Krey rather implies that his oils are made from brown coal tar or asphalt. Thus, beginning with the last line on page 4225, I read:

"I may be permitted to describe to you today this process which found another use for the heavy oils by working them up into light oils (benzine and lubricating oils); lubricating oils with higher viscosty than is possessed by the brown coal tar oils or asphalt oils, by distillation under higher pressure."

That, again, is a clear implication that he is talking about brown coal tar oils or asphalt oils. Although it is not a direct statement this is implication that he is working with such oils, and that his gas oil is derived from those sources is borne out by the fact that he says on page 4225 that he did his work in the Webau factory, and that his process was patented on February 27, 1886.

Turning to plaintiff's exhibit 246, on page 4210, I quote from the first line:——

The Master: What is that, the patent?

The Witness: This is a description of the operation of the Webau factory that he refers to.

"The Webau factory uses as a starting material the tar of the Saxonian brown coal."

And on page 4212 of this same exhibit he states that the products of distillation of this brown coal tar include light [fol. 888] gas oils and dark gas oils; and on the next page he mentions the fact that he has developed an apparatus for cracking these heavy oils by distilling under pressure.

Q. Just at this point: this distillation you are speaking of there is the preliminary, original distillation of the brown coal tar oils at atmospheric pressure?

A. The products of distillation which he refers to are from the atmospheric pressure distillation, which he says in this reference he has developed an apparatus for cracking by distilling under pressure; that is, cracking the products of the first distillation by distillating them under pressure.

Also referring to his patent, in plaintiff's exhibit 97, he says, page 1289, the second sentence of the patent:

"Thus, for instance, the heavy dark paraffin oils (brown coal tar oils) have heretofore been used only for the preparation of oil gas or the production of low-grade lubricants." Which you will note accords exactly with his de-

scription in the first part of the plaintiff's exhibit 247. This clearly identifies his gas oils and heavy, dark paraffin oils as brown coal tar distillates which are, as I have already stated and indeed shown, by Krey's own work, are extremely dirty stocks and entirely different from Burton's low-boiling relatively clean cracking intermediate distillates from petroleum, even though they have the same name, on account of the fact that both of these types of oil happen to be used for making gas.

It is simply another illustration such as plaintiff's expert gave of the different uses of the word in Europe and in America.

Q. Now, you have already stated that all of the many stocks disclosed in the prior art bulk pressure distillation processes cited by the plaintiff was dirty stocks. In view of the importance of this point I wish you would enumerate the stocks disclosed in each of these processes?

A. In discussing these processes, it should be noted that all of the processes are of European origin, and therefore the terms used are, in general, used in their European sense.

In many cases it is evident from the context that where petroleum is mentioned they are talking about shale petroleum or shale oil in the European sense.

In other cases it is doubtful. For the context you cannot tell what they do mean, but again, it is presumably shale oils [fol. 889] of European origin in accordance with their usage in calling shale oil petrolum.

The specific stocks in these references are as follows: In the Young process the only stocks specifically disclosed are of shale origin. However, in his introduction he does refer to distillates from coal and a petroleum, which may or may not mean well petroleum. In any case, however, all these stocks are notoriously dirty stocks.

DeWar & Redwood discloses the cracking of a reduced crude oil. Laing——

Q. Just a moment. Just how is it that they disclose that, Mr. Wilson?

A. DeWar & Redwood say in their patent, beginning line 20:

"In distilling mineral oils, such as natural petroleum or similar oil made from shale, coal or other bituminous substances, in order to separate the lighter oils suitable for

lamps and other purposes from the heavier oils, there is frequently a very large residue of heavy oil."

And he goes on to discuss the handling of these residues after removing gasoline and kerosene.

Q. And that which is employed as an antecedent process is the skimming process that you described yesterday?

A. Yes, which is particularly applicable to stocks with high asphaltic content, such as many of the Russian oils.

Laing also discloses cracking of reduced crude oil. His specific example is of crude residuum having a 14 gravity, which is a very heavy product.

Mr. Wiles: Just a minute. I just want to call attention, here, to the fact that the reduced crude, which you brought here as typical, is 25 gravity, whereas this is very much heavier, even than that—14.

The Master: Who brought that here?

Mr. Wiles: In the exhibit case—this specimen is 25.7 whereas Laing is 14.

The Witness: Which is a much heavier gravity.

Pielsticker's stock appears to have been crude petroleum, and it is not certain whether he used it in the American, or European sense. That it was very heavy is clearly brought out by the character of the tar, because even with the light cracking, to produce kerosene, such as he had in mind, his tar was so heavy, that he introduced the use of steam ejectors, to get it out of the still, lest it not flow out by itself. [fol. 890] He also refers specifically, to some cases where the crude oil contains water, so that there seems to be little doubt but what the only stocks which he had in mind, were wet, or dry crude petroleum. As to whether he refers to American, or European petroleum, or European shale oil, is not clear.

Q. His residence is given in the patent as being European, is it not?

A. Yes, though he had both *both* an American and British patent.

Q. He was an Englishman if I recall it?

A. Yes, sir. In Barbet, the only stock which reaches the cracking still is reduced crude from petroleum, again, probably, although not certainly, of shale origin.

In the case of the Riebeck process, they experimented with a great many stocks, in different operations.

You will recall that there were about ten or twelve different references, which worked with the Riebeck process. It is only ncessary to say that all those stocks which were used in the semi-commercial work, were very dirty, being primarily, brown coal tar oils, shale oils, crude petroleum, oil gas tar, and so forth.

The Riebeck and other processes, were also tried on a laboratory scale on a good many special stocks, in order to try to arrive at something of the nature of this cracking reaction, to study it scientifically, and see what was really taking place, and in this work were tried out various kinds of fish oil, paraffin wax, refined lubricants—lots of chemical compounds, referred to in "Exhibit No. 102." I don't know from personal experience with regard to any of those stocks, whether they are clean or dirty, but it is not a matter of concern, because they were all so comparatively rare and costly that no one would have been interested, either then, or at any time since, in using them to make gasoline at a profit. They are scientific curiosities, and experiments.

Q. Please tell us the effect and the importance of condensing under pressure in a pressure distillation process for gasoline manufacture such as that of Burton?

A. The function and advantage of pressure condensation in a gasoline-making process is that it permits the gas to be separated from the distillate while the gas is still under pressure. You will recall that in the Burton process the condensate runs into a drum while it is still under pressure and the gas is taken off at the top and the liquid is taken off at the bottom. By taking the gas off from the liquid while it is still under pressure, its volume is much smaller, being compressed by the pressure, and the amount of the light ends of gasoline which it will carry off is, by the laws of physical chemistry in practically direct proportion to its volume.

That is not merely a theoretical thing, but a thing which I personally and others have quantitatively demonstrated. So that it is a great advantage to separate the gas from the liquid while its capacity for carrying off the light ends is limited by the fact that it occupies a small volume. The net result of this action in the Burton type of process is an increase of approximately eight per cent in the amount of the gasoline left in liquid form, rather than coming off with the gas.

Furthermore, this eight per cent is of the most desirable materials, as plaintiff's expert has pointed out. The light ends are the most desirable part of the gasoline, and a part of the gasoline which is practically free from the gum-forming constituents, which tend to be present more in the heavy end. So that it is proper to say that condensation under pressure improves the quality as well as increases the quantity of the distillate in a gasoline-making process, providing, of course, the gas is actually separated from the liquid while under pressure. It is possible to condense under [fol. 891] pressure, and yet lose all the advantage, if, as some did, you permit the separation to take place after the pressure had dropped to atmospheric.

Q. Referring to the prior bulk pressure distillation processes cited by the plaintiff, do you find any of them that showed a process in which pressure condensation had a similar function?

A. No, I do not. In the processes of Young, Pielsticker, Riebech and Barbet the condensation of the distillate tock place at atmospheric pressure. In Laing, he had some distillation and condensation in these intermediate condensers I spoke of, which were necessarily under pressure, these intermediate condensers which reflux back to other sections of the still; but there was no separation——

The Master: Final separation?

The Witness: There was no final separation of the gases and the liquids at that point. His final condensation he specifically recommends he carried out at atmospheric pressure.

In some of the laboratory glass tube experiments where the distillation was carried in a sealed tube, heating one end and cooling the other, condensation was of course necessarily under pressure, but there there was no control of the pressure and no attempt to separate the gas from the liquid while it was under pressure. It was a sealed vessel. If the gas was gotten out, as it presumably was, by breaking the glass, of course most of the gas and the liquid would rush out together and the separation would take place at atmospheric pressure, thus losing the utility that was present in the Burton process.

This leaves only of the prior art bulk pressure distillation processes the Dewar & Redwood process, which we will have to consider a little more in detail. In any process like that

of Dewar & Redwood, here the still and the condenser are in free and open communication and the liquid level at some intermediate point in the drum, it is necessary that enough gas be formed by the operation to saturate the distillate at the operating pressure and the temperature prevailing in the receiving drum, because if that does not happen, if the system is not making enough gas to saturate the liquid in the drum, then the pressure will tend to fall and generally when it tends to fall more distillate will come over and it will form more and more rapidly—set up a sort of vicious cycle. If, on the other hand, you are making more [fol. 892] than enough gas to saturate the distillate, the pressure will tend to rise, unless you release the gas through a separate valve, which is of course what Burton does.

Q. That is what Dewar & Redwood does?

A. When they have an excess. This whole mechanism of pressure condensation in a process that of Dewar & Redwood was correctly explained by plaintiff's expert, but he assumes that in any pressure-cracking process there was always enough gas produced to saturate the liquid, even at pressures of several hundred pounds per square inch, the truth of which assumption he could only determine by experiment. As a matter of fact he was very far from being correct.

In connection with the other suit which I mentioned, I personally operated the Dewar & Redwood process built as closely as possible after the patent, with a cracking tube, as I recall it, eight or ten inches in diameter, to check up on this point. Now first as a sort of background, even in cracking to make gasoline, where the amount of the distillate is smaller, (since we try to keep the kerosene in the still) and the amount of gas is greater because of the heavier cracking which takes place, frequently there is not enough gas left to saturate the distillate in a single still at ninety-five pounds. That is one of the reasons why we find it necessary to use this high line, so that when there is a temporary deficiency in the gas which one still produces, another one will make it up. However, on the average the amount of gas produced in cracking to make gasoline is considerably more than enough to saturate the distillate. But if we were operating so as to take off a large quantity of distillate, namely, the kerosene distillate, and do less cracking, characteristic of our kerosene-making process,

it is quite probable that there would be nowhere near enough gas, possibly only ten or twenty per cent as much gas, in relation to the distillate, as in a gasoline-making process. And, as I say, I tried this out personally in the still built according to the Dewar & Redwood patent, and found that in general the amount of gas produced when cracking to make kerosene was not even sufficient to maintain the pressure as low as sixty pounds, and therefore, of course, there would not have been enough gas to saturate the liquid at still higher pressures.

It is probable that this deficiency of gas to keep the liquid saturated on the average in cracking to make kerosene; is one reason why it was not adopted by the prior art proc- [fol. 893] esses and could not have been used until Dewar & Redwood made the suggestion of pumping in outside gas to supply that deficiency.

Plaintiff's expert, to be sure, maintained that Dewar & Redwood's disclosure of pumping in gas was only meant to take place in the first part of the operation and to have the function of preventing the distillation at that point. Such a conclusion seems to be of doubtful validity, even if we had nothing more than the statement in the patent cited, plaintiff's exhibit 103, read from line 56 of the patent, page 1359 of the book of exhibits:

"We charge and keep charged the space in the boiler or retort and condenser that is not occupied by liquid with gas under considerable pressure. It may be with air or it may be carbonic acid gas or with other gas that cannot act chemically on the matter treated."

As I said, however, plaintiff's expert maintains that as soon as cracking begins the still at all times makes an excess of gas and that this excess would be allowed to escape through the valve D5, which is shown on the patent drawing, which case of course, if we were releasing an excess of gas, no sensible person would keep pumping in gas. But as I have shown, that is actually not the fact in cracking to make kerosene, at least at pressures above 60 pounds. Of course, we do not know at what pressure Dewar & Redwood planned to operate. That Dewar & Redwood's experience was similar, however, in that they very seldom made an excess of gas, and therefore, generally needed gas, is indicated more clearly in the British patent to these same pat-

entees which is quite similar to the American patent but contains a very illuminating sentence.

I quote from page 3, beginning line 27 of this patent. This has to do with the question of whether, in point of fact, they continually produced an excess of gas and bled it off through the valve D-5, and so did not need a pump in while they were running, or whether in their experience they do not have enough gas to keep the distillate saturated.

I read, beginning line 27:

"In some cases the operation is accompanied by produc-
[fol. 894] tion of permanent gas which, being incapable of condensation, would accumulate and produce abnormal pressure. A suitable cock or valve should therefore be provided to allow escape of such gas from time to time."

In other words, it is only in some cases or occasionally that Dewar & Redwood found they produced an excess of gas, and that means, of course, that the rest of time enough gas to saturate the liquid must be pumped in from outside.

The third alternative of a perfect balance being kept at all times is, as plaintiff's expert has admitted, untenable. It will tend to shift to one side or the other of the balance inevitably.

Mr. Wiles: The British patent No. 10,277 of 1889, corresponding in substance, I think, except for the single sentence to the American patent already in evidence, is offered as Defendants' Exhibit 404.

(The said patent so offered, was admitted and read in evidence, was marked "Defendants' Exhibit 404" and the same is herewith returned.)

Q. Now, having shown that in the Dewar & Redwood process it is necessary to pump in gas a large part of the time, please show how this affects the function which pressure condensation has in the Burton process?

A. As I pointed out, the whole utility of pressure condensation in a process like that of Burton, is that it enables the excess of gas produced to be withdrawn while it is still under pressure instead of being released saturated with the distillate at atmospheric pressure.

Now, if on the average you have to pump in gas in order to saturate the distillate, and that gas goes out with the

distillate and separates from it at atmospheric pressure, instead of reducing your losses you increase your losses when that gas comes away; and if you use a very soluble gas, like carbon dioxide, as Dewar and Redwood suggest you might increase them quite markedly in such an operation as that.

In other words, it is only when you are withdrawing more gas from the valve D-5 than you are pumping in, in total, that you get any gain and that clearly is not the type of operation that Dewar & Redwood had in mind. The definite tendency of their method of operation would be to increase, rather than decrease, the vaporization losses.

Q. Did John Laing make any reference to the difficulties of this method of condensation?

A. Yes, John Laing, who wrote this in England three or [fol. 895] four years later, apparently had this process in mind when he said that a prior method with the valve beyond the condenser gave trouble because "when the oil under pressure escaped from the valve it came away surcharged with *with* gas, like aerated water when the cork is drawn and that meant losses of material as well as risk from fire."

Laing, accordingly recommended condensation at atmospheric pressure in his patents. It is thus apparent that neither Laing's papers nor Dewar & Redwood's patent disclose a process in which pressure condensation had the utility that it did in Burton, namely, of increasing the quantity and improving the quality of the distillate.

Q. Even assuming that under some exceptional conditions the Dewar & Redwood process could have produced and released excess gas throughout the cracking operation so as to produce a small saving in distillate, would its functions in the Burton process have been present?

A. No, it would not. Apart from the fact that the amount of the excess gas in such a process, and hence the amount of saving would be negligibly small as compared with that in the Burton process, we must remember that Dewar & Redwood were making illuminating oil and the very light ends which Burton wanted most to save and preserve in his gasoline-making process were undesirable parts of illuminating oil, and would have to be removed from it before it was a marketable product. In other words, even if they had a microscopic increase in quantity

of distillate that would not have been an improvement in quality, which is probably the most important part or certainly a very important part of its function in the Burton process.

Q. Just what was the function of pressure condensation in the Dewar & Redwood process?

A. The function of pressure condensation in the Dewar & Redwood process was exactly as stated in the patent, "to get the benefit of more regular vaporization and condensation than could be obtained" with an ordinary loaded valve which was rather unsteady in its operation.

Q. Is this advantage of importance in the practical art today?

A. No, it is not, because the same steadiness and close regulation of the operation can be secured in other quite different ways. Furthermore, the mere fact of going to larger scale operations tends to steady down an operation [fol. 896] like that and make it quite a simple matter to obtain regulated distillation and condensation, quite different from that obtained with the loaded valve which released a gush of vapors and then takes some time to close and makes an undesirable, unsteady operation.

Q. If the advantage that Dewar & Redwood stated is the sole advantage that could be obtained, would pressure condensation, in your opinion, be employed today in the practical art?

A. I doubt it very seriously. The extra expense and danger which John Laing mentioned would probably have outweighed the trifling advantage of steadier distillation, since it would not have been any steadier than could have been obtained by other means.

Q. Now, I want you to compare the Burton process on the one hand and the Dewar & Redwood process on the other, both as to the mechanism and function of pressure condensation, just briefly.

A. In both cases the mechanism of pressure condensation is not far different, because we have in Burton the chance to introduce a small amount of gas in case the still is not making quite enough at any particular time; but in the Burton process we have the function not present in Dewar & Redwood of making a marked increase in the quantity and improvement in the quality of the product

formed, which function alone is the reason for its use in the practical art.

The Master: Is not that result instead of function?

The Witness: If pressure condensation did not have the function of increasing the yield of distillate and improving the quality of the distillate, it would not be used in my opinion in the practical art today. The undesirable features of having to have the receiving drum in the house where the men are working, under pressure, the increased danger of leakage and the fact that the distillate comes away charged with gas and has to be handled specially would, I think, outweigh these advantages.

Of course, another advantage of condensation under pressure which Dewar & Redwood did not have but which might make it a desirable method, is the single point control of a battery which Clark later developed in his high line patent and which is only possible with pressure condensation. But that was not present in either Burton's original process or in Dewar & Redwood.

[fol. 897] The Master: You said that these were in batteries of three in the Burton——

The Witness: No, batteries of twelve as a rule. The earlier stills in batteries of twelve, the later stills in batteries of ten. But more than that are connected to a single high line.

The Master: I am speaking now of the Burton shell still.

The Witness: Batteries of twelve.

Mr. Wiles: The three was in one of the references that was read this morning, I think, your Honor. The Crew reference spoke of them in batteries of three—crude stills.

The Master: But was the gas line in the Burton shell still hitched up to each one of the units in the battery?

The Witness: Our first row, I think, has either 60 or 120 stills on its high line. I think 60 stills on the high line. In other words, five batteries. The battery division simply marks——

The Master: Is that the high line in this original Burton patent?

The Witness: No.

The Master: I am speaking of that.

The Witness: There is nothing in the original Burton patent regarding the high line. That was a later invention.

The Master: I understand that, but was there a gas line shown here?

The Witness: There was a gas release.

The Master: Only a gas release?

The Witness: A single gas release line. No high line.

The Master: But where did you get your additional gas to increase pressure in this Burton patent?

The Witness: The fact that you are taking off a smaller amount of distillate, which of course in itself increases the ratio of gas to distillate, and the fact we were doing much more cracking, and therefore getting more gas, which also increased the ratio of gas to distillate.

The Master: Then it was confined to the self-contained gas in the system?

The Witness: Yes, he was considering only the self-contained gas in the system.

Q. When you are cracking to make gasoline you have relatively more of that?

A. Yes, you could operate stills under fairly constant weather conditions for a good long time without trouble, but occasionally you would find, especially if a sudden rain- [fol. 898] storm, or wind, came up that you would slow down the rate of distillation, and then you would fire hotter to bring it up, and then get into an unsteady operating condition, where for a time you would not have enough gas unless you had a lot of stills connected together.

The Master: What is perplexing me there, you referred to it as a function when it occurred to me that it was a result. Assuming that both are using a gas pressure and one of them is utilizing it in the production of kerosene, where he can waste his light ends, and the other is utilizing it but he is saving his light ends. Is there a difference in function at all; isn't the function the function of the gas pressure, in one instance availed of for a certain purpose and in the other utilized for another purpose?

The Witness: Well, I have always considered those as being different functions or different results. I do not distinguish very closely between those two words.

The Master: You have got to distinguish between function and result. If you have a function in a machine or apparatus or means for producing a distinct result, but one uses his results for a certain desired object and the other uses his results for another object, the function is the same, the means is the same, it is only a difference in result.

The Witness: Well, but in the example which you gave, you used the same result for different purposes, but if we are talking about different results——

The Master: You use the means for different purposes.

The Witness: You use the means to produce different results.

The Master: Yes.

The Witness: Well, that is what we are doing here; we are using the means to produce different results, and the purpose for which the condensation was carried out.

The Master: I did not know whether there was a jump in your logic there. I may be entirely wrong, but it is worthy of thought.

Mr. Wiles: That is a very interesting question that I shall want to argue at some length.

The Master: I think you had better. You had better clear me up on it; I may be woll-gathering, but——

Mr. Wiles: I had not thought of it in just that way, and I am very glad to have that brought to my attention; but while we are on it I want it very clearly shown that in the [fol. 899] main—neither do I want your Honor to overlook the fact that in the main Dewar & Redwood operation the thing is not here at all.

The Master: The what?

Mr. Wiles: The result is not here at all, or the function is not here at all. Because in the main——

The Master: The object of Dewar & Redwood was east and the object of Burton was west.

Mr. Wiles: Note that the first half of the witness' testimony was that normally Dewar & Redwood would not have any gas to release at all, so that neither function nor result would be present. Now I might put this question, because I don't know whether it is covered:

Q. I wish you would compare, Mr. Wilson, in quantity, the amount of light ends that would be saved by pressure condensation in the Burton process making gasoline and

the Dewar & Redwood process in making kerosene. I do not know whether you have covered that or not.

A. Well, normally Dewar & Redwood would not save anything by their method of condensation. In fact, they would lose a little more, which of course would not be a matter of particular concern, since they did not want the light ends. But their normal operation would be to lose a little distillate by pumping in this extra gas to entirely saturate the distillate; whereas Burton saves eight per cent of his distillate and at the same time gets a more desirable distillate.

Q. And even at the time when they have to release a little gas, how would that compare in quantity with what Burton saves?

A. I cannot conceive of their having to release enough to make a difference of as much as a per cent in the amount of distillate recovered. Of course, as I stated, I agree that the mechanism of the two operations is very much the same. I am not denying that for a moment. The mechanism, or the means, as you call it, is very much the same as I have just testified.

The Master: Of course, if the mechanism was there complete and one skilled in the art used it to get a little different result, he would be just in the same situation as if he screwed up his rolls instead of making hominy to make corn meal.

Mr. Wiles: I think that is a question for your Honor [fol. 900] rather than for the witness. Of course, this is only one element.

Q. Mr. Wilson, I wish you would take these several points and summarize briefly Burton's several contributions to the modern art of practical gasoline manufacture.

A. Burton discovered that gas oil, an available stock boiling mostly between six and seven hundred degrees Fahrenheit, could be cracked in a large direct fired pressure still and produce commercial quantities of a marketable gasoline, without producing a hot bottom, and to this he added the step of pressure condensation, which improved the quality and increased the quantity of his distillate.

He also found and disclosed a pressure and temperature which were suitable for handling this stock for the pro-

duction of gasoline, and yet maintain a black bottom condition.

These disclosures of Burton were not merely a chance combination of conditions which happened to work out well, but as a matter of fact they marked out very closely the limits within which the art worked for a great many years. In fact during the whole period from 1912 to around 1918, in spite of the enormous increase in the demand for gasoline and the extensive amount of research work which a great many refineries were carrying on to get other processes or improved processes, this was the only process in commercial use during that entire period.

The Master: Deliberate selections after experimentation for a definite result.

The Witness: Correct. This in many ways is the best indication of its tremendous advantages over anything that was in the prior art, in the fact that these others who were working on the same problem had to work for six or seven years before they were able to develop, with all that was shown in prior art, any other process which was capable of coming anywhere near competing on a commercial scale with the Burton.

Q. Did the practical Burton process in time depart at all from the narrower preferred limits specified in his patent?

A. Yes, it has. Burton's narrower limits of sixty to seventy-five pounds and 750 to 800 degrees were quite suitable for his stock, that is gas oil boiling from six hundred to seven hundred degrees, but certain gradual changes [fol. 901] have taken place in the industry which has made it desirable to shift slightly from those narrow preferred directions. In the first place, instead of kerosene boiling from 350 to 600, as Burton said it did in his time, the decreasing demand for that as compared with gasoline has, as I pointed out, narrowed up the cut on both ends, it now boiling mainly from around 400 to 550.

This left over more material in the range from 550 to 600, and also the heavy ends of pressure distillate are now available for cracking. The fact that this lowered by 50 or 75 degrees the boiling point of the stock, which it became

desirable to crack, is the primary reason that in actual operation the pressures are now about twenty pounds higher and the temperatures about 25 degrees lower than the preferred limits disclosed by Burton. Of course, such minor adjustments of pressure and temperature to meet minor changes in the composition of the stock are readily within the scope of what anyone skilled in the art would do, once the proper limits had been pointed out for the gas oil stock, and the Burton process had been demonstrated as being sound in theory and reliable in practice.

The Master: That is one of the vexing things right here, that question of the selections of temperatures, being within the choice of the operator.

The Witness: It is quite a different thing, your Honor.

The Master: Which is not, as I understand it, practicable?

The Witness: There is a great difference between dropping 25 degrees to make up for a small change in the volatility of the stock, and picking out a proper temperature from this tremendous temperature range of 650 to 2,000 degrees, with no sign-posts to guide one. It is a very much simpler job to make slight shifts once you have got your bearings, than it is to explore a wilderness.

Q. Which, if any of the references cited by the plaintiff of date subsequent to 1912 fall in the class of bulk pressure distillation processes?

A. The processes of Graefe, Exhibit 144, of Bacon, Brooks and Clark, Exhibit 122, and of Edwards, Exhibit 142.

Q. Please state briefly what these references show?

A. Graefe's patent, plaintiff's Exhibit 144, describes work which was carried out in Germany, while Bacon, [fol. 902] Brooks & Clark described work which was carried out at the Mellon Institute of Industrial Research at Pittsburgh, a very large scientific institution engaged in industrial research. Both of these patents were filed in 1913 and issued in 1915. They are especially interesting as indicating the logical outgrowth of the prior art, without the benefit of Burton's disclosures and discoveries. Both of them had come to the point of recognizing the coke difficulties, but instead of avoiding those coke difficulties in cracking by Burton's expedient of selecting a clean stock, they both simply employed the old standby of the prior

art, that is reduced crude oil, and that is all they were considered cracking.

Graefe tried to overcome the coke troubles by the use of a lead bath surrounding his retort, and Bacon tried to do it by using a vertical side fired still with the coke settling chamber at the bottom. It is also true that both of them worked at pressures which were too high to be commercial, on a very large scale, at that time, although they might be operated today. Particularly too high considering the dirtiness of the stocks. It is also interesting to note that neither of them got the advantage of condensing under pressure, Graefe carrying out his condensation at atmospheric pressure, while Bacon, Brooks and Clark condensed under pressure, but allowed both the gas and the distillate to pass out through the same valve and separate it at atmospheric pressure, thus losing Burton's function of separating the gas from the distillate under pressure.

The third patent in this class, which issued after 1912, is that of Edwards, and Edwards clearly builds on the inventions of Burton, Clark and Humphreys, the latter of which I have not yet described, and adds some of his own improvements designed to make continuous operation possible. So far as I know, however, these suggestions of Edwards were never put into commercial use.

Q. Do you know happen to know whether Edwards is a Tidewater man?

A. I would judge that from——

The Master: Well, Edwards could not be availed of for prior art at all, unless he carried it back beyond the filing date of Burton. There is no suggestion of that in this case.

The Witness: I think that this was probably cited with regard to some later patents.

Q. Now, what other main lines of developments were [fol. 903] there in the art of bulk pressure distillation for gasoline manufacture as practiced by the defendants after Burton?

A. The main developments in the practical art working along the general lines of the Burton process fell into four general classes as follows:

First, the discovery of new clean stocks suitable for cracking in a direct fired pressure still, in addition to the one which Burton himself discovered.

Second, the discovery of new methods of minimizing the coke difficulties which would enable either dirtier stocks to be handled, or else the cracking to be carried down a little further before it had to be stopped on account of the dangers of hot bottom troubles.

Third, improvements in the methods of condensing and returning the reflux condensate to the stills.

Fourth, what might be called mechanical improvements which were on the whole rather minor in nature, but were kinks which increased the ease or the safety or the cheapness of operation.

Q. Now, will you please describe those improvements in gasoline manufacture which you said were based mainly on the discovery of new clean stocks?

A. The Burton patent 1,105,961 and the Chamberlain patent No. 1,221,790.

Q. What was the essential contribution in the Burton patent No. 1,105,961 which we have generally spoken of here as the re-run patent?

A. Burton found, as the art has found since that time, and as I have already pointed out, that merely starting with a clean stock did not solve the cleanliness problem, because as cracking progressed the once clean stock became a dirty stock and began to deposit increasing amounts of coke. Of course, again by saying "a dirty stock" I do not mean a stock which contained dirt as such, but a stock which, on further cracking, would become dirty. Merely taking out this stock, settling it and allowing it to deposit suspended matter, as plaintiff's expert suggested, would not cure the difficulty with that stock. It is something more far-reaching than that.

There are three general ways to handle this problem of the increasing dirtiness of the stock. In the first place, you can work on a small enough scale so that regardless of how dirty it is you keep on cracking down to coke and use a small enough vessel with thick enough walls to stand the strain when very large amount- of coke are produced.
[fol. 904] The second method is to withdraw the stock and sell it as fuel oil for what it will bring. Both of those methods were suggested in the prior art.

The third method that which Burton suggested is quite a different one, and that is to withdraw this stock from the

still, take it over to an atmospheric pressure still and distill it down to coke. The resulting distillate he found was then suitable for further cracking. In other words, by this process of distilling—down to coke residue or heavy tar residue at atmospheric pressure, you could eliminate from the stock all the coke forming constituents and deposit them in a place, in an atmospheric still, where no matter how much coke was formed it did not matter. It would deposit something like four per cent of coke there, and only about a tenth of a per cent over in the pressure still.

This invention nearly doubled the amount of stock which was available for cracking and was, of course, therefore, a very important contribution.

The Master: Was that mentioned in the patent at all?

The Witness: In No. 1105961 or the original——

The Master: No. 1,055,707.

The Witness: No. 1,105,961, is the re-running patent.

The Master: Does he there describe this taking of the partially distilled matter, running it through an atmospheric still?

The Witness: Yes, sir, and then recracking under pressure the distillate that was thus produced.

Q. The step that you have just described is essentially the step described in the patent in question?

A. Exactly.

The Master: That is the essential step?

The Witness: Yes, sir.

The Master: In what you call the re-run patent?

Mr. Wiles: The re-run patent.

The Master: That begins at line 77 on the first page.

Q. Now, were the results of the operation——

A. Excuse me. I call your attention to the fact that the two drawings represent, first, the pressure still, in which he makes the pressure tar, and, second, the atmospheric pressure still, with the characteristic pressure and vacuum relief valves, in which he re-runs at atmospheric pressure, and then puts it back into the other still for second cracking.

[fol. 905] Q. Now, were the results of these operations which Burton disclosed in this re-run patent——

The Master: He, of course, did introduce an entirely new step in the entire operation.

The Witness: Exactly so.

Q. Were the results which you have stated as inherent in the process described in this Burton re-run patent, predictable by one skilled in the art?

A. No, they were not. This tar that was left in the stills, was a new product, which no one knew anything, particularly about. It would not have been surprising, from even what we know today—well, I can hardly say that—but it certainly would not have been surprising if Burton had found that all of this pressure tar was so dirty that none of it could be used again for cracking, with success. Whether or not the operation of re-running it and taking out the distillate, would sufficiently improve the cleanliness of this stock, was a thing, which, so far as I can see, was entirely unpredictable.

Furthermore, even though it had been known to be a clean cracking stock, you must remember that it was a material that had not yet cracked to make gasoline, after 12, 15 or 20 hours digestion, under cracking conditions. In fact, most of these products were themselves products of cracking. Whether it would crack under Burton's conditions, was certainly unpredictable—in fact, most of us, I think, would have said that it was improbable that it would have cracked satisfactorily to make gasoline.

As a matter of fact, it does crack at about half the rate, but particularly by raising the pressure to 95 pounds, but even at 75 pounds it does crack, and makes a very satisfactory gasoline, and gasoline of practically as good quality as that made from virgin gas oil.

Q. You say half the rate—you mean half the rate as compared to what?

A. Half the rate as compared to virgin gas oil at a given temperature. Of course, by raising the temperature a little you can increase the rate.

Q. Now, I wish you would compare the disclosures of this Burton re-run patent, No. 1105961, to the prior art tube cracking processes, which were cited against it by the plaintiff's expert?

A. In comparing with these tube cracking processes, I want to again emphasize the essential feature of Burton's

[fol. 906] invention, which was to remove the stock outside of the cracking zone, where coke did harm, and to make whatever coke had to be produced at atmospheric pressure, and then put the stock thus cleaned up, back into the pressure cracking zone. The mere re-running of the material around and around, through a tube cracking process, without cleaning it up, is not in any sense, an anticipation of what Burton did. His problem of operation was entirely different from that.

It is also true that the tube cracking processes make nothing really analogous, or similar, chemically, to Burton's pressure tar. Those which operated at the higher temperatures, characteristic of tube cracking operations produce a different type of pressure tar, smaller in amounts, generally heavier in gravity, and containing more aromatic hydrocarbons, in fact tars such as are formed as oil gas tar, in the cracking of oil to make gas.

They make nothing, though, that really resembles this Burton pressure tar, which was made by cracking conditions extending over periods of many hours.

The Master: They did not take the steps taken by Burton?

The Witness; That is right.

The Master: Nor did they get the results that Burton did?

The Witness: Nor did they operate on the same kind of material that Burton did.

Of course the failure of the cracking art to clean up their stock in tube cracking operations was due to the fact that they can tolerate more coke in their systems, which are capable of being run red hot. There the dirtiness is not so vital as it is in a commercial bulk pressure still—simply another illustration of the radical difference between the problems of the two arts.

Referring specifically to the disclosures—the only ones of the tube cracking art, which did do any recirculation, were Schuchow & Gavrilow, and Kittle, who merely re-circulated the dirty tar around and around in the system, and Testelin & Rennard, and Noad who re-circulated the heavy part of the distillate. Of course, in the tube cracking process the distillate is a very large proportion of the total material. You don't have the Burton proportion of ap-

proximately 50% distillate and 50% tar resulting, as a rule.

Now, those distillates may have been clean, or may have [fol. 907] been dirty—I don't know—but if they were clean, the only reason why they were clean was that the dirty part of them had been cracked to coke, the first time through the operation—they still put the coke in the same place, that is in the cracking part of the apparatus—the place where we don't want it, and the place where Burton didn't want it, so that he disclosed some other method of making recracking possible.

Oglobin re-cracks his tarry heavy naphtha.

Barbet, in "Exhibit No. 124," re-circulates his heavy tar, and possibly some of his heavy distillate, but, as I pointed out, there is no operation in them, resembling that of Burton's.

Q. Now, in connection with the processes of Noad, and Testelin and Rennard, plaintiff's expert put forth, on page 1023, of the Printed Record, the theory that these processes combine the first two steps of the Burton operation, in that they both form and distill their pressure tar in the cracking zone. What have you to say on that point?

A. It isn't necessary to say much more. Of course, combining those steps simply lost the whole utility of what Burton was trying to do, which was to separate the coke formation of the two parts of the process, and take the bulk of the coke outside of the cracking zone.

Now, it might have been that that was not necessary in the tube cracking art because of the greater ability to tolerate coke, but it was certainly very far from disclosing anything of what Burton did.

The Master: If practiced later in time that would not be an infringement of Burton's patent?

Mr. Wiles: If anybody wants to try to run a bulk pressure still that way, he will blow his bottom out in about fifteen seconds. That is just analogous to running clear down to coke in a pressure still.

Q. What do you find in the prior art of bulk pressure distillation processes—and I will not include Barbet, for the moment, because I want to take that up in detail—which bears on Burton's re-run invention?

A. I don't find anything which comes even close to disclosing Burton's operation, the essentials of which I have already described.

Some of the prior art—particularly the laboratory processes, on a small scale, ran clear down to coke in the pressure still, and some then simply put back into the cracking [fol. 908] zone, part of this distillate, and re-cracked which again means leaving all the coke in the cracking still, which is just what we want to avoid.

Furthermore, if Burton could do that, there would be absolutely no point in taking the stock out of the still and cooling it down and heating it up again, and cooling it down and putting it back into the pressure still.

Burton stopped his operation where he did, because he had to, and then found a method for making that tar suitable for his kind of an operation.

The Master: He stopped his cracking operation to clean out his coke?

The Witness: To clean up his stock.

The Master: To clean up his stock by eliminating his coke?

The Witness: By eliminating the constituents which tended to form from coke on further cracking. It so happened that this could be accomplished by the operation of distilling down to coke at atmospheric pressure. It could not be accomplished by the settling process.

Q. In cases where the prior art processes did have a liquid residue, how would it compare with Burton's pressure tar?

A. Since these prior art processes are almost entirely kerosene making processes, their tar would be quite different, much less decomposed, and, in fact, in many cases, as they pointed out, suitable for lubricating oils, which is quite different from Burton's badly decomposed product, which is practically worthless for any purpose except for fuel oil, or for re-cracking after it is cleaned up.

Q. Now, with reference to the operation described by Barbet, in "Exhibit Nos. 136 and 141," plaintiffs expert stated, in answer to a question by the Master—page 431 of the Printed Record—that this process was identical with the process of the Burton re-run patent. Now, is this conclusion of the Plaintiff's expert correct?

A. No, it is not.

The Master: Why not?

The Witness: It is very far from being correct, but I will have to go into a very detailed explanation, to say why.

The Master: Go ahead.

Mr. Wiles: I am going to go right through t at, because that is a real one.

Q. Which of the Barbet patents shows his process most completely?

A. Patent No. 322265, "Exhibit No. 136," shows the operation most completely. In that patent there are some parts which are shown above the column, H, and the reflux, R. Those are for the separation from the crude oil, of what they called the light essences, after which the remaining light reduced crude descends into the lower part of the apparatus. The parts above the column H, or the reflux condenser R, are not at all pertinent to the question of what gets back to the pressure still. They are all to handle the light products—lighter than kerosene, or anything with which we are concerned in this part of the case.

Q. So that we can just forget this for the time being, and, permanently, as far as this question is concerned? I have had prepared, as nearly as possible, an exact duplicate of the lower part of the drawing in the Barbet patent, and I have checked over all the essential points, myself, just as carefully as possible. The drawing produced here is a photostat, not from the original, but from a draftsman's drawing that we had made. This was made by a draftsman, rather than by a photograph, because the lines were not sharp enough to photograph properly. But this is to a scale and is exactly the same as the patent drawing, as near as it can be made.

[fol. 909] A. The contention of the plaintiff's expert was that some of the material which was in this pressure tar, here, in the pressure still, F, passed through the line, Y—I think. Passed through the line, VZ, into the bottom of the tower, Q, where it flashed into vapor, and——

Q. Just a moment. You said "flashed"—the tower, Q, is at atmospheric pressure, is it not?

A. Yes, sir.

Q. So that when the liquid comes in there, due to the release in pressure, there will be a large amount of evaporation, immediately?

A. Yes, sir. Of course, if this still is going to be full, there must be a valve in the line which Barbet failed to show, but it is clearly evident that it must have been there —in order to regulate the withdrawal of tar from the pressure still, and, therefore, on the release of pressure a considerable quantity of the material in the pressure tar would be vaporized.

The contention of plaintiff's expert was that part of the vapors that came off from the pressure tower at that point, were condensed out in the upper part of the tower, and withdrawn separately, but that the illuminating oil, plus an unmentioned gas oil cut, passed over through the re-flux condenser, R, and through the line, tt, into the tower, H, and that then this hypothetical gas oil, not mentioned at any point, by Barbet, descended through the plates of the tower, H, into the still, M, at atmospheric pressure, where they were not vaporized, and finally around through a pump, X into the pressure still, P.

Q. And that was the pressure tar distillate, and its course, which he said accomplished this similar, or identical process with Burton's?

A. Yes, sir.

Q. Now, what is your position—just as briefly as you can state it—and later I want you to go over it in detail?

A. My position is that in this method of operation there is no gas oil cut—that is, there is no material between Barbet's paraffine distillate, and Barbet's heavy illuminating oil; that he only——

The Master: You mean to say that there can be no——

The Witness: That in Barbet's method of operation [fol. 910] there is none—as he describes it, there is none. He might have changed his operation, conceivably, in such a way that there would be one, but in his operation as disclosed, there is no gas oil cut, and that only illuminating vapors, some of which are fairly heavy, pass over through this pipe, tt, and are separated out in the three lanterns marked 6, 7, and 8.

And furthermore, that even if there was a little material, heavier than heavy illuminating oil, which accidentally came over in this line, tt, it would inevitably be removed, or taken off from the lower part of the column, through

the valve, L, the condenser G, the cooler, K, and tapped off at the point 9, and none of it could get back into the pressure still.

The Master: That is, none of it could get into the still, M, and consequently could not get back into the pressure still?

The Witness: Even if it dropped down into M, it would be boiled off again, and out through this valve—all of the light products that go into M, are distilled off through this system, G, K and 9.

Q. Now, omitting the parts which you have not shown on this drawing, first tell us, in detail, what the column H, and the still, M, are for, and how they work them.

A. The crude petroleum—suppose I put pencil marks on this for the purposes of clarity, to just indicate what is flowing through these various pipes.

The crude oil, which has been freed from its light essences, and therefore, is a reduced crude, from which only the very lighter products have been taken off, passes down through the line, to the left of p, and into the re-boiler, as it is called, G, which is a vapor heat exchanger, the heat from the vapors given off by the still, M, being given up therein, and thereby boiling off the kerosene from the reduced crude oil, which passes through the heat exchanger in separate pipes.

You will notice the small line coming into this exchanger, and the large line leaving it, indicating that vapors have been formed therein. The illuminating oil vapors which have been formed and the unvaporized portion of the crude pass into the center of the column, H, and the vapors, of course, rise in that column, and the liquids descend.

The column, H, is apparently a bubble tower, of more or less well known design, and we need merely to keep in mind just what takes place in such a bubble tower.

In such a tower, with the source of heat at the bottom, [fol. 911] and cooling means of one kind or another and at the top, there is a gradual gradation of temperature.

There are pools of liquid on each plate, and the top pool is the coolest, and the bottom pool is the hottest. Furthermore, the vapors which leave any plate are always lighter than the liquid which is on the plate.

What happens in such a column is that as the vapors arise from the still and pass through the successively cooler plates, the heavier products are condensed and allowed to run back to the still—whereas, the lighter portions pass on through.

Furthermore, if liquid products are put in, as they are, at r, and pass down through there, as they get to the hotter and hotter plates, the lighter constituents are distilled off, and only the heavier ones pass down.

The Master: That is the real object of the design of the bubble tower?

The Witness: Yes; as a result of this operation we have a separation at the top of the column of very light distillates practically free from all heavy ends, and at the bottom of the column the comparatively heavy distillates stripped reasonably free from their light ends, and we also have at intermediate points of the column some less well-fractionated cuts, not as sharp as the cut which comes off of the top, yet which gradually grade up as you come down the column.

Now, as I say, what happens to this crude coming in is that most of the illuminating vapors—that the illuminants which have been vaporized in the re-boiler G, pass upward and are segregated on the three plates s, s', and s' s, the lightest illuminants coming off through the cock s; the intermediate illuminants coming off through the cock s', and the high or heavy illuminates coming off through the cock s''.

You will note we have here three different grades of illuminants, the heavier as we shall see later, probably quite a heavy product, a good deal heavier than ordinary kerosene, remembering of course that Barbet was working to get a maximum yield of kerosene.

The material that was not vaporized in the reboiler G therefore enters the boiler as liquid pass down through the plates of H, wherein, as Barbet says by "the methodical boiling on the successive plates of H provokes the total evaporation of all lighting oils, and even a little of the paraffin products."

That is of importance in two senses. First, we find that all of the illuminating oil is dist-lled here, and second, it em- [fol. 912] phasizes clearly that the next thing which is

heavier than the heaviest part of the illuminating oils which Barbet was making, was paraffin distillate. If the next heaviest thing would have been gas oil, of course, he would have logically said, "evaporating off the gas oil" or "some of the gas oil." As I pointed out, however, when you are running for a maximum of kerosene, and particularly when, as in this case, you take off three grades of varying quality, the light quality, the intermediate and the heavy cut. It is very probable that a great deal of the gas oil cut goes into there (indicating 5").

As I further pointed out, in making paraffin distillate a good deal of what might go into the gas oil later is taken off with the paraffin distillate in order to make it possible for it to be pressed. Therefore, it is quite common in the art to have absolutely no primary gas oil cut when you are running for a maximum of kerosene. There is no question but that from this and other points that that was exactly the thing Barget meant. The only thing he was considering which was heavier that his illuminating oil was the paraffin distillate.

That is further borne out by the fact that when we get down into the still we get a large, further amount of vapor formation before any of the liquid can get over in the pressure still, and that the material which is there boiled off goes through the line L, through the re-boiler G, through the cooler K.

You will notice that this cooler K is much larger than the other coolers which they are shown, and of course the re-boiler G is also a partial condenser. So, there is no question but what Barbet meant to condense a large part of his distillate therein—that is what he was designing it for and the condensate clearly described as a product which will solidify and which necessitates keeping the cooling water warm in order to prevent the wax from separating out and clogging the condenser, showing that this was really a cut of paraffin distillate and this was necessarily the next heavier cut than his heavy illuminating oil.

As further indicating the character of his heavy illuminating oil in another patent, the Barbet patent No. 390,558, which is in the book of exhibits page 1747, he says with regard to the separation of illuminating oils that as a general rule two products are sufficient, one heavier than 45.4 Baume, (converting it into Baume scale), and one lighter than 45.4 Baume.

[fol. 913] And he says that the product which is heavier than this limit comprises, "sometimes fifteen per cent or more of heavy oils, the presence of which impairs the brilliancy of the flame." In this case he has taken three cuts and it is reasonable to suppose that in his heaviest cut he has even more of the heavier oils. Therefore, the only thing which we would conclude is that all of the gas oil either went off into the heavy illuminating oils or was unquestionably taken off in G as the lightest part of the paraffin distillate which went into here (indicating).

In view of that, the only thing which can get over into the still, P, (indicating) is a little of the heavy part of the paraffin distillate and the other constituents of the crude oil which are heavier than that, because all the lighter portions have been taken out by this careful fractionation before the oil leaves the still, M.

There is one more indication that Barbet was considering no gas oil cut, nor even contemplated a gas oil cut. That is his statement as to what happened in the tower Q and Q'.

"The illuminating vapors (which are formed in this cracking operation)" leave the condenser R by the tubulator *t* and by the tubes *tt'* to the column HH' where they are refined and classified, which is just as I have indicated. The paraffin liquids condensed run back by UU'. That is, this small line here (indicating). In other words, the vapors rising from Q consist of the illuminating products and the next heavier material, which is paraffin liquids; and he describes carefully the making of the split between those two products. The illuminating vapors pass over here (indicating) and the paraffin liquids returning here.

He further says that these are freed in Q prime of all illuminating products. Now, if he had been concerned about and gas oil cut there, of course, he would have said, "they are free from the gas oil cut," if there was any gas oil cut to be separated, and even if there had been that gas oil cut sent over into this column H—and by speaking of there being no gas oil cut I did not mean that there is nothing which boils in the gas oil range, but that Barbet was putting this material into other cuts. It could not possibly have gotten back into the still R because it would have been vaporized before the paraffin distillate, which is clearly indicated as being taken off at the bottom part of the column H.

[fol. 819] Q. I do not know whether you made this clear or not, but you can state whether or not it was the common

practice when you were running for the maximum yield of kerosene, as one would be likely to do in Barbet's day, to have no gas oil, to put all the oil either in the kerosene or paraffin distillate.

A. Yes, especially if you were making a poor grade of kerosene, such as Barbet evidently was. As I pointed out, he was making——

The Master: That is, if he was separating out a poor grade from his better grade.

The Witness: If he was taking off in his kerosene, not only light products, but heavier products.

The Master: Yes. In which of those tanks M or P, does he start his original material?

The Witness: His original crude oil comes down through this line here (indicating); this line to the left of *p* as indicated.

The Master: Yes.

The Witness: And after passing through the reboiler G goes into and down through the lower part of the column H, where all of the illuminants and a little of the paraffin products are distilled, and goes into the boiler M where a large amount of material is distilled off, and then and only then, into the -ressure still P.

It would be hard to conceive of a more effective method of insuring that nothing lighter than light paraffin distillate ever got into the pressure still than Barbet's scheme.

Q. Now, suppose, for the sake of argument, that a trace of pressure tar distillate did work its way back into Barbet's still through the course that plaintiff's expert outlined; would the thing then have the essence of the Burton re-run invention?

A. No, it really would not, because this was a clean stock patent. This is a new clean stock, as I have already pointed out; the mere presence of a small amount of clean stock with a large amount of dirty stock does not have, in any sense, the function of these clean stock inventions. In other words, you could not carry out extensive cracking on a large scale to make a good yield of gasoline without getting very quickly a red hot bottom.

Q. Just state, as nearly as you can describe it in modern terms, Mr. Wilson, just what it is that actually enters the pressure still in Barbet?

A. It is reduced crude after you have taken off light paraffin distillate, and all products lighter than light paraffin distillate. In other words, it includes——

The Master: If it starts with crude it necessarily contains all the coke-forming constituents.

The Witness: All the heavy coke-forming constituents which have made trouble in all these processes.

The Master: Which Burton was trying to separate out?

The Witness: Yes, sir.

The Master: By his process?

The Witness: Yes, sir.

Q. And even if a little of this hypothetical gas oil worked its way down into M, it would go into the pressure still P and mix with a very large body of this dirty stock?

A. Yes.

[fol. 820] The Witness: I might just mark on here and then we will have it, "illuminating oil"; this is the heavy and this is the light paraffin distillate (indicating).

The Master: This line that runs from the bottom of M over to X is not marked, but being drawn off of the bottom of M, would it draw off the heavier part?

The Witness: Not appreciably different than if it were drawn off any place below the liquid level. Of course, if it were drawn off to include vapor it would be lighter, but in any still like that, the material drawn from the bottom would not be appreciably heavier than that drawn from the top of the liquid. It would, however, contain a little more sediment, if there were any sediment in the still—that is suspended matter, as distinct from the dissolved hydrocarbons.

Q. Now, Mr. Wilson, will you have one of these duplicate drawings of this drawing that you have been working from marked by a draftsman with legends so they will be legible when reproduced, and produce it here so we can offer it?

A. If we have both of them there will be no chance——

Q. Then I will offer them both.

Mr. Wiles: With the Court's permission, I will have these legends properly written on one of these charts, put in one showing the patent drawing as it is and then the other one with the legends on it, and I will offer it when Mr. Wilson brings it in with the legends on it.

The Master: I think that is rather meticulous but if you want to do it, as long as there is no objection, you may put it in. I cannot see the idea of putting a patent drawing in which was put in by the plaintiff. Of course, you have got this enlarged. I will permit it to go in with full liberty to them to show any deviation but if you simply introduce one with the legends on it, why, it will save all that trouble.

Q. Will you have the legends fixed and we will put it in.

A. I did not have them put on because I did not want any question raised of what was in the patent and what I had done.

The Master: With that restriction you can put it in, although it seems to be useless.

Q. What was the essential contribution of the Chamberlain patent which you have referred to as a clean stock patent?

A. Chamberlain in his patent really made a very comprehensive survey of just what of the ordinary cuts of crude around the refinery could and could not be cracked in a commercial size direct fired pressure still, to make a reasonably high yield of gasoline, and his findings are very much in accordance with the experience that we have acquired even to date. That is, he showed that along with Burton's gas oil you could combine light paraffin distillate and heavy paraffin distillate about in the proportions in which they occur, although you could not crack the latter two materials by themselves you could add them to the gas oil as it came off and have a stock which, while not so clean as the gas oil, you could crack if you stopped a little sooner, or if you used Humphrey's false bottoms, or something like that. He also pointed out that the heaviest products, the two heaviest cuts which come off from crude—that is the slop cut and the still wax cut—could not be cracked successfully, even if they were mixed with large amounts of gas oil, although he admitted you might have a trace of those present without spoiling the process. And that represents just about the actual facts as we know them today. Apparently the heavier products that [fol. 321] come off near the last ends of this and other coking operations cannot be successfully cracked, they have a tendency to be very dirty stocks.

Q. Plaintiff's expert in his cross-examination, pages 994 to 1003, practically admitted the novelty of Chamberlain's

specific stock, that is, gas oil plus light and heavy paraffin distillate, but he asserted in his direct testimony, page 510, and he substantially reaffirmed in his cross, that Chamberlain in the last paragraph of his patent specification so broadened his description of his stock as to include practically any crude oil. What is the fact as to the oils included within the last paragraph?

A. The last paragraph of Chamberlain's specification in plaintiff's exhibit 182, U. S. Patent No. 1,221,790, reads as follows:

"In reciting in claims following that the oil is 'free from still wax and like heavy matters of sticky character and from elements yielding the same or coke', a practical rather than a theoretically perfect freedom is to be understood. It suffices, for example, for the oil operated upon to be distillable in an externally heated still of the dimensions hereinbefore mentioned for still *a* (charged six-tenths full) under a super-atmospheric pressure of 75 pounds to the square inch at the aforesaid rate of two per cent per hour to the extent of fifty per cent or more by volume of its component hydrocarbons which have viscosities above seventy seconds at 100 degrees Fahrenheit, without heating the still bottom to redness."

The last clause, of course, is the important clause. The contention of plaintiff's expert that this statement in the last paragraph so broadened Chamberlain's disclosure as to include almost any residuum and specifically crude residuum from Pennsylvania or many of the mid-continent oils, is not true in point of fact. As I have already pointed out, every one wants to use these reduced crudes, and many people have tried them, in our own company and other companies, both on a large and small scale, and they have all been found to give a hot bottom before they were anywhere near fifty per cent off.

You are fortunate to get five or ten per cent off of a crude, even a light crude, before getting a hot bottom, even under Chamberlain's conditions. If this statement does broaden Chamberlain's disclosure at all, it is just to the extent that [fol. 822] he specifies—that is, when he says you cannot use slop wax or still wax, he implies a practical rather than a theoretical freedom, that is you might put in a little and sitll withstand this test.

The fact that these crude oils or residuums cannot be used in pressure stills has been known to everyone skilled in the art certainly for the last ten years. Chamberlain himself stated the facts very accurately on line 86 of page 1 of this same patent, from which I quote:

"With a view to increasing the yields of naphtha and burning oil (kerosene) hydrocarbons, it was long ago proposed to distill under superatmospheric pressure oils which are composed of or contain higher boiling hydrocarbons, as crude petroleum, for example, or undistilled residues of crude petroleum after removal therefrom of crude naphtha and water-white burning oil (kerosene) distillate (with or without obtaining more or less of the mixed distillate hereinabove mentioned). Such proposals seem to have little, if any, practical value; because, for one reason, distillation of crude petroleum and its undistilled residues under sufficient pressure could be carried on with safety in stills of practical size only to a comparatively slight extent on account of overheating the still bottoms."

You could not have a more clear stat-ment of the facts than this. Burton in his Perkin Medal Address, to which plaintiff's expert has referred, also pointed out the fact that these crude oils have been tried and found impossible to use, and that such operations of bulk pressure distillation on a large scale could only be carried out with clean stocks, such as gas oil.

The position of plaintiff's expert on this point, which is of course a very vital point in the whole development of the cracking art, that reduced crude could be run in Burton stills, and his apparent belief that reduced Mexican crude was being run, in the Burton stills at Providence, can only be explained on the basis that he entirely disregarded the literature and the practical development of the art since 1912, and relied on these prior art statements which we have been discussing, as to the possibility of cracking these dirty stocks in direct fired stills. As a matter of fact, the only available stocks with which I am familiar, which will meet this test of Chamberlain's, are the two which Burton disclosed, namely, gas oil, and the distillate from pressure tar, and those disclosed by Chamberlain. Kerosene is also [fol. 823] a clean cracking stock, but could not be cracked

at a commercial rate at the pressures which Chamberlain mentioned.

Q. Is Chamberlain also correct with regard to the necessity for the dissolution of paraffin distillate?

A. Yes, he is. Paraffin distillate alone is too dirty to crack to the extent which he mentions in direct-fired shell stills.

The Master: Mr. Wiles, I do not get the connection at this point, that you are trying to make—connection or distinction—between Chamberlain and Burton. What is the idea of the reference to Chamberlain at this point?

Mr. Wiles: Well, I have passed Burton for the time being, and Chamberlain is attacked, and it comes right along. Mr. Wilson says we have three patents——

The Master: You have passed Burton?

Mr. Wiles: And the Burton re-run——

The Master: You are pointing out the distinctions between Burton and Chamberlain?

Mr. Wiles: Yes.

The Master: And what Chamberlain was trying to do?

Mr. Wiles: Sure.

The Master: Is it your intention, then, to take up the reference in respect to Chamberlain?

Mr. Wiles: There are none.

The Master: There are none?

Mr. Wiles: No. It is admitted by the plaintiff's expert that the Chamberlain stock is new but he says the last paragraph so broadens Chamberlain's description as to include all kinds of oil, that any old oil will meet Chamberlain's test. Now, that is a perfect——

The Master: Therefore it is not new.

Mr. Wiles: Sure. Now, that is a perfectly cold question of fact.

The Master: Oh, yes.

Mr. Wiles: And any expert who says, as Dr. McKee did, that he saw them running Mexican crude in a still in Providence, when he inspected it, he would say you naturally could run any old thing, but I will prove before we get through that he did not see Mexican crude being run in a Burton still, that he never saw what he said he saw, and that it could not be done.

The Master: I just wanted to get the connection.

[fol. 824] Q. You stated that there were two other inventions after Burton, which depended, essentially, on the discovery of clean stocks, and you discussed the Burton re-run patent, and finally, at the last, the Chamberlain patent. I think that was practically the conclusion. That comes down, then, simply, as stated yesterday, to a question of fact. Will the ordinary crudes meet the Chamberlain test, or not?

A. There was one point that I might not have made entirely clear, yesterday, and that is that a great share of Chamberlain's patent is devoted to describing this coking still operation, as commonly practiced in the art, and which I described, and which the Burton operation presupposes as a preliminary step, and, furthermore, that Chamberlain's work was done in a Burton still, simply to try to extend the number of available stocks that could be cracked in that still which Burton had demonstrated could be operated satisfactorily on gas oil.

Q. This antecedent coking process that Burton presupposes, and Chamberlain describes, is the same one that plaintiff's expert, on cross examination, and plaintiff's witness, Gifford, both stated had been in common use for a long time?

A. Yes.

Q. Now, leaving these stock type of inventions, but still confining yourself for the time being, to the Burton process, what were the further developments in the direction of minimizing the coke difficulty? That was another one of the groups of improvements which you said came in.

A. The finding of clean stocks was, of course, the biggest step ahead in putting cracking into operation on a commercial scale, but it was still, and still is today, desirable to [fol. 825] crack dirtier and dirtier stocks, and crack them further and further before removing the pressure tar. So that a very large part of the development of the cracking art has been in the direction of minimizing coke difficulties in pressure stills, in order, as I say, to make it possible to use either dirtier stocks, or to run longer before the material must be withdrawn from the still.

Q. You are still on that job, too, today, aren't you?

A. Yes, we are. The first development along that line was the Humphreys' false bottom plates, which are described in the Humphreys patent No. 1122002, which was

offered in evidence a short time ago. You will recall that——

The Master: I remember quite distinctly, the false bottoms.

The Witness: Humphreys shows two sets of hinged false bottom plates, one above the other, to prevent coke settling on the bottom. This makes it possible to run quite a little longer, and use quite a little heavier or dirtier stocks, than would otherwise be possible.

Q. Now, plaintiff cited, against this patent, the Henderson patent—"Exhibit No. 81"—would the same disclose——

The Master: You mean, cited the Humphreys patent?

Mr. Wiles: The Henderson patent.

Q. Would the same disclosure in this Henderson patent accomplish Humphreys' purpose?

A. No, it would not. While the two devices are superficially similar, in a general way, it will be noticed that the Henderson device has a wide opening down the middle, between his trays.

Q. Just state for the Master, not having it in mind——

The Master: I have a plain vision of the Humphreys' device—his false bottom device.

Q. I wish you would state what Henderson had—just to bring it back.

The Master: The Henderson false bottom is not an entire closure, of course, of the bottom of the tank, the circulation going down to the fire box, with pans setting in there. It is a very obvious experiment, and yet patentable, if it showed an advantage. Now, what is Henderson?

A. You have before you the Henderson patent, which shows two pans on each side, with a space both at the center and at the sides, to permit free circulation to take place around the pans. Now, it is conceivable that in an operation that produced very little coke——

[fol. 826] The Master: Let's see—I have a cross section, Fig. 5. Now, go on.

A. (continued). You will note that there are openings left both at the sides and in the middle, to permit circulation, which circulation is supposed to take place, coming

down through the middle, and being impelled by jets from the steam pipe, 16, in the still bottom, flowing up along the sides of the still, and passing the outside edges of the pan.

The Master: In Fig. 5, I assume at 44 and 45, those are simply studs to hold the pans in there, with openings between them. Is that right?

The Witness: Yes. Now, in any operation where the amount of coke formed is comparatively small, and where you can use steam for producing violent agitation—such devices might, conceivably be helpful, or fairly effective for the purpose, but in a pressure cracking operation, such as Humphreys directed his attention to, the use of steam is out of the question, because the effect of steam is to just nullify the effect of pressure. We put pressure on the oil in order to keep it in the still and prevent cracking, and if you blow steam in it tends to sweep the oil out of the still in vapor form. So that the use of steam is out of the question with the Humphreys double plates. The use of any such large openings as shown here, would entirely defeat the purpose of the invention, and I will point out that in the early days some experiments were made with single false bottom plates, and while those were of a little value, the butt joints between the sections of the plates allowed so much coke to scale down and localize on certain spots, that the advantage was comparatively small, and only by using the two double rows of plates, which Humphreys shows, and by staggering the joints, so that what fell through the top joint, would be caught on the second set of plates, made it possible to get any great prolongation of the run. So that the plates which Humphreys shows would not be practical in any pressure process where steam could not be used.

The Master: Well, of course I assume that even with Humphreys' pans, or false bottoms, it does not completely operate to prevent the deposit of coke upon the bottom of the still?

The Witness: No, sir. Such coke as is formed below the plates is deposited on the bottom, but it does hold away from the still the great bulk of the coke which is formed up in the main body of the oil.

[fol. 827] The Master: Yes, and I can conceive that possibly these pans, forty-two, of Henderson, might catch some of the coke.

The Witness: Yes, sir, they might catch some of the coke, but they could concentrate the bulk of it right at the hottest point of the bottom, because the circulation, even in the absence of steam, would be a circulation, probably in the same general direction, tending to go up along the sides and come down in the center, and, as I say, that would concentrate the coke on the hottest part of the bottom, where we least want to have it.

The Master: That, I think, would be one of those cases, where the invention is almost entirely predicated upon utility.

Mr. Wiles: That is right.

Q. You mentioned, here, the impossibility of using steam in a pressure process, and to complete the record at this point, I want to ask you whether you agree with the statement of the plaintiff's expert, that in general the injection of steam and the use of a vacuum, are equivalent in this art—in the oil refining art—the art of distillation, generally?

A. In most respects they are equivalent, in so far as they both lower the boiling point and permit the distillation to take place at a lower temperature than would otherwise be the case. Of course they are not equivalent in the amount of agitation that may be produced by injecting steam at a particular point.

Q. So that if you put steam in you just work against your pressure?

A. Yes.

Q. And get just the opposite effect?

A. Yes, sir.

Q. What was the next invention or improvement, in the direction of removing, or minimizing the coke difficulty, permitting longer runs and slightly dirtier stocks to be used?

A. The next improvement, and one of very marked importance, making possible a good deal larger step in advance, than the Humphreys false bottom plates, for example, was that of Clark, described in his tube still cracking process, patent No. 1119496, which is already in evidence.

Clark's invention consists essentially in circulating a rapidly moving stream of oil from and back to a bulk supply, which is held in a lagged drum, to which no direct heat is applied.

The value of the feature consists in the fact that by carry- [fol. 828] ing on the major part of the cracking in a lagged drum, it is possible to allow the carbon to settle out on unfired surfaces, where it does no particular harm, and then by maintaining a rapid circulation of oil you will minimize the accumulation of carbon in the heating tubes.

The Master: Why do you call it a lagged drum?

The Witness: That is an insulated drum, insulated from the surroundings.

The Master: You say it is insulated from its surroundings?

The Witness: It is insulated from the air. In other words, instead of applying the heat to the bulk supply of oil, to get pressure, which involves a hazard of coke depositing on the bottoms, he applies the heat to a circulating stream, which goes from and back to the bulk supply of oil in an unheated drum.

The Master: The verb "lag," puzzles me. In what sense is "lag" used?

The Witness: Lagging is the customary term for insulating, or insulation, in the oil industry.

The Master: I have heard that verb used, but I always heard "lag" used in respect to time, or electrically.

Q. Heat insulation, you mean?

A. Yes, heat insulation. That is a rather generally used term.

Q. In this sense this lagging simply means covering with a heat insulating layer, does it not?

A. Yes, sir.

Q. And you usually use an asbestos, or some such layer, outside of the drums, do you not?

A. Yes.

Q. And that layer is several inches thick, so that the heat dissipating through it, is as small as possible.

A. Yes.

The Master: Well, is there any provision of a slowing up of the stream through this drum, the flow through this drum?

The Witness: Yes, it necessarily slows up being a large volume of oil held there. When the stream comes from the small pipe, where it has been rapidly circulating into the large drum, the rate of circulation slows down very

markedly and most of the cracking takes place in the bulk supply of oil, which is in the unheated container. It is thus found possible to settle out most of the coke instead of its staying in the stream and going around and depositing on the heating surfaces.

Q. Now, what was the first commercial embodiment of this invention and its practical value?

A. The first commercial embodiment of this invention differed from the patent in producing the circulation by a thermosiphon action, in a structure similar to that in a water tube boiler instead of pumping the oil through the heating coil, as illustrated in the Clark patent. Of course Clark's patent is a process patent, not an apparatus patent, and the essential features of the process are preserved in the other form of construction. This form of construction actually used is shown most clearly in the Burton and Clark patent No. 1,448,254, except that certain features are added there. I also have a drawing which I now produce showing in diagrammatic form the arrangement of the Clark tube still for carrying out the Burton process and it also shows the Lewis and Cooke bubble tower which I have not yet described.

Mr. Wiles: This is offered as defendants' exhibit 405.

[fol. 829] (The diagram so offered, was admitted and read in evidence, was marked "Defendant's Exhibit 405" and the same is herewith returned).

Q. Now, on the Master's question which was put a minute or two ago, how does the speed of the circulation through the tubes compare with that through the shell?

A. The speed of circulation through the tubes is—I can calculate it accurately; that is the best way to do.

The Master: So far, there is nothing different in this diagram of Clark's patent, except instead of your diagram showing the lagged drum off in another part of the system, you have just simply put it over your fire box.

The Witness: Correct.

Mr. Wiles: Then you are able to omit the pump, your Honor.

The Master: There would not be any difference whether you positioned it one place or another.

Mr. Wiles: No.

The Witness: The biggest difference, from a practical standpoint, is the fact that this gets away from the use of a pump and accomplishes the circulation by virtue of the rise of temperature and the formation of bubbles of oil in the tubes, much like boiling water causes an automatic circulation to take place.

The average velocity through the tubes is something like twelve to fifteen times as fast as that through the cross-section of the still, but the circulation through the tubes, we feel quite certain, is not uniform, because the bottom row of tubes, where there is the most danger of overheating, takes up the most heat and therefore has the greatest thermo-siphon action; so that the rate of circulation in the bottom row, which is the one we are most concerned about, is probably twenty or thirty times as great as that through the still.

The Master: Why do you use the term "thermo-siphon"?

The Witness: Thermo-siphon is the expression——

The Master: I mean as applied to this diagram.

The Witness: When we have two sections of a single body of liquid and heat one side of the U or V or whatever shape it may be, we tend to expand the liquid and decrease its density, and also to form bubbles, which further decreases the density.

The one leg which is not heated, therefore, being filled with heavier liquid, tends to start the downward circulation, which rises upward through the light leg and which results in a continuous circulation caused by the application of heat to one side. Of course, if both sides were heated equally, there would be no such action.

The term is very frequently used in automobile engines. Some have pump circulation to force the oil around and others simply rely on the formation of steam bubbles and the heating up of the water and making it lighter to cause a circulation up through the engine and down through the radiator of an automobile engine.

Q. That latter action, therefore, is called thermo-siphoning.

A. Yes.

The Master: Thermo-siphoning.

The Witness: The advantage of this scheme is——

Excuse me, I notice one other error in this drawing. When we come to consider details we may see others.

The Master: You had better point out all you can because your adversary will point out some, probably, before you get through.

[fol. 830] The Witness: They are minor points that are noticed when we focus our attention on them. The back leg extends about a foot further up into the still than is indicated in the drawing.

The Master: There are no letters on here. You mean the right hand?

The Witness: The right-hand leg.

The Master: The right-hand ascending tube?

The Witness: The right-hand tube through which the liquid descends.

The Master: You called it the descending tube.

The Witness: Yes.

The Master: Caused us to think the fluid passes through it descendingly.

The Witness: That extension is also shown in the Burton and Clark patent to which I referred—that is, it takes off from a point about a foot above the bottom of the still.

Q. You used the term "back" Mr. Wilson, and that may be used many times in the course of the testimony.

The Master: Because you are assuming that the front is the firing end of the apparatus, the left hand here (indicating).

The Witness: Yes.

Q. That fired end of the still is the front end and the opposite end is the back end.

The Master: Yes.

A. Yes. May I correct the exhibit in that additional respect?

The Master: Yes.

(Witness corrects Exhibit 405.)

Q. Now, will you continue with your description of the practical value of this contribution of Clark's?

A. The practical value of Clark's contribution is best shown by the fact that, with the Burton shell stills it was

not found profitable to feed in any oil after the run started, and they only charged 8,250 gallons.

With this form of construction the stills are made somewhat larger since they are not exposed to direct fire, namely, ten feet in diameter by around thirty feet long, and they are charged with about thirteen thousand gallons of oil and they feed during the run about seven thousand gallons additional. So that we have a total charge (operated on and cracked for the production of thirty to thirty-five per cent of gasoline) which is more than twice as great as was possible in the shell still form of construction. Naturally, the cracking of so much larger amounts in a single unit and for a single heating and cooling of the still makes a marked decrease in the cost of operation.

The Master: Well, the chief thing about this is the depositing your carbon away from your fire contact; is that it?

The Witness: That and the fact of the rapid circulation through the fired surfaces—very rapid circulation. Clark found it tended to minimize very markedly the deposition of carbon.

The Master: I want to get this a little clearer. Aside from the changing of the position of this lagged drum to a place over your fire box, is there any change from the original Clark disclosure; was there the same circulation back through the tubes from the lagged drum in the original Clark disclosure?

[fol. 831] The Witness: The original Clark disclosure shows rapid circulation from and to the lagged drum and the maintenance of a cracking temperature in the lagged drum by circulating the oil through a heating tube at high velocity. The differences are in the position of the drum and in the use of the thermo-siphon instead of a pump to cause the rapid circulation.

The Master: In the original Clark device you would have thermo-siphoning, would you not, if you had the connections?

The Witness: No, the rate of circulation is determined by the amount the pump forces through.

Q. Just the difference you were bringing out a moment ago with reference to the automobiles; for instance, in one

case the pump, and the other they circulate by the thermo-siphon method?

A. Yes, and, as I pointed out, one advantage of the thermo-siphon is that it gives the most rapid circulation in the tubes which are exposed to the hottest fire, whereas the pump circulation gives uniform velocity, approximately, throughout the whole heating coil.

Q. Generally, how have these Clark tube stills compared in number with the original Burton shell stills, particularly in the more recent construction?

A. The more recent construction has very much predominated in the construction of tube stills. There are a few who have felt they would prefer to standardize on the shell type of still, but it is now generally recognized that the tube still is far preferable to the shell still in economy and efficiency, and even in respect to safety, because while we get more fires in a tube still due to splitting of tubes, they are not nearly such bad fires as when a bottom splits, or anything of that kind happens with a shell still.

Mr. Royce: May I ask how many years you are covering by "more recent construction"? That is the way the question was.

The Witness: That would apply to construction since around 1918 or 1919, although many tube stills were built before that time.

Q. I wish you would please compare the Clark process, or this tube still patent, as I have usually referred to it, with the references cited against it by the plaintiff's expert?

The Master: Now you are going back to the original Clark patent?

Mr. Wiles: Yes, the Clark patent.

The Master: You have skipped over to the Burton and Clark with this diagram.

The Witness: No, I simply said that the actual form in which the Clark patent is embodied in practice is that shown in my diagram, or that shown in the Burton and Clark invention, rather than the form shown in the drawing of Clark's process patent for cracking in a large unheated drum, and maintaining the temperature by rapid circulation from and through the drum. We have not yet

taken up the Burton and Clark patent except to refer to that figure as showing the form which the Clark invention actually took—except, of course, the original Clark invention did not include the use of the funnel, which was added in the Burton and Clark invention.

[fol. 832] Q. Will you now please compare the Clark process of the patent you have just been discussing, No. 1,119,496, which I have normally referred to as the tube still, with the reference or references cited against it by the plaintiff's expert.

A. The only reference cited against the Clark process was plaintiff's exhibit No. 130, a patent to De Clerq. Clark, as I pointed out, describes a pressure-cracking process in which a large body of oil is held at a cracking temperature by circulating from and to it a stream at a high velocity, through a heating zone, whereas De Clerq merely has an apparatus patent for a still construction for distilling tar at atmospheric pressure, without any cracking, at least without any desired cracking. He is trying primarily to avoid cracking, and if he did have cracking it was merely incidental to his distillation, which he wanted to take place with a minimum of coke deposition.

De Clerq's purpose was merely to heat up the body of tar and drive off the light ends and then stop. In other words, his operation stopped where Clark's really began, because Clark wanted to hold his drum at a high temperature for a very long time and maintain that temperature by circulating back and forth, whereas, as I say, all DeClerq discloses is a particular form of apparatus which is suited to the distillation of tar at atmospheric pressure, gradually heating up the main body of tar to the point necessary to distill off the light ends of which he wished to dispose.

The Master: Where is that DeClerq patent?

The Witness: That is plaintiff's exhibit No. 130. Superficially, you will note, the apparatus is roughly similar to that disclosed by Clark, but Clark's is not an apparatus but a process patent. I will point out that DeClerq also fails to show Clark's rapid circulation, or to say anything about the speed of circulation, and he also fails to disclose carbon deposition in the drum, presumably because, of course, he was not trying to crack.

The Master: Could DeClerq's operation be used with the same results as Clark's?

The Witness: Is that a question?

The Master: Yes.

The Witness: No, DeClerq's operation could not give the same results as Clark's for a number of reasons. In the first place, DeClerq had no pressure; he presumably did not have an apparatus which was capable of standing any such high pressure as Clark prescribes. In the second place, DeClerq says nothing about the rate of circulation through his tube, which Clark pointed out was very important. And, in the third place, even if you added rapid circulation and pressure to DeClerq's disclosure, he would fail in one important point, that he withdraws from the very bottom of his drum, so that any carbon which did settle out would get back into the flowing stream and over into the heating coils, where you did not want it, whereas Clark you notice takes off from the side——

The Master: Oh, Clark's takes off—pipe 2 here takes off, truly, from the side, but from the side at the bottom.

The Witness: Yes, but there is a dished bottom in both cases, which would serve to catch a very considerable amount of coke and a very important amount of coke, but DeClerq takes from the very bottom of that dished bottom, whereas Clark takes off above the dished bottom from the side; and also in the practical embodiment of the Clark still the inlet to the back tubes is always at least a foot above the bottom of the still.

Q. Do you, as one skilled in the art, consider that the use of the tube still type of apparatus was an obvious expedient to use in bulk pressure distillation in 1914?

A. No, I do not. I do not see how anyone can predict [fol. 833] offhand whether putting the heat into small tubes like this would give more or less trouble from coke. In one way you will see it would have a tendency to concentrate the difficulty in a very much smaller amount of surface exposed directly to the fire—that is, the bottom row of tubes. Furthermore, from my own experience I can say if it were not for the point which Clark showed, of taking the outlet above the bottom and allowing a definite

space for coke to settle there, you actually coke up quicker in a tube still than you do in a shell still.

You must both slow up the rate of circulation very markedly and avoid taking off the circulating stream from near the bottom, if you are going to make effective the deposition of coke away from the heating surfaces, instead of simply carrying it around again to the heating surfaces.

Furthermore, particularly in 1914, it was not known whether the coke formation took place in the main body of oil and settled out, or took place only on the heating surfaces. The indications which Humphreys had were probably the first indications in that direction, that it was a settling proposition, but there were many people who believed that since coke was always found on the heating surfaces, that that was where it all formed.

The Master: Was Clark a continuous stream operation or a batch operation?

The Witness: Clark was, in accordance with some definitions that you asked for sometime ago and which I will give later, a semi-continuous process, in that he fed the stills but did not draw tar continuously; he circulated continuously and he fed—I believe this patent does not show the feeding, but I am not positive on that point.

The Master: I did not read it closely enough to get it, and I thought you could tell me quicker than I could read it.

The Witness: Yes, I am not sure whether Clark shows feeding. I am thinking of the practical operation of the process. If he does show feeding, it is not one of the essential features of his patent.

The Master: He had not claimed it?

Mr. Wiles: No.

Q. But this operation was sufficiently cleaner that you could feed it, whereas you could not and did not feed the Burton shell stills?

A. That is the situation.

Q. Had you concluded this question of the obviousness of this expedient?

[fol. 834] A. I might add that, of course, water tube boilers which again bore a rough relationship (although differing greatly in the proportions of size of the drum relating to the amount of tube surface) were of course old

in the art and were for an entirely different purpose, namely, to greatly increase the amount of heating surface available for heating up the water, not for producing a change in one lagged chamber by supplying the heat in another chamber. Increasing the heating surface was not the problem that had to be met in the pressure still cracking art; it was the proposition of doing the heating away from the space where the coke deposition took place.

The Master: Is there any citation of any tube still for distilling petroleum or petroleum charging stock prior to Clark?

The Witness: Under pressure?

The Master: Under pressure?

The Witness: No, that is a very good——

The Master: No citation of that kind?

The Witness: No citation of that kind. DeClerq is the only citation, which is an atmospheric pressure distillation process or apparatus.

I had not finished my answer regarding the water tube boilers. I think the most significant indication that the expedient was not obvious is that, while both plain boilers like Burton used and water-tube boilers like Clark used were very well known in what we might call the art of steam making, everyone who worked with a pressure distillation process uniformly adopted the simpler type of construction, and in fact Young specifically recommended against using a water-tube boiler, and said not to use a boiler of the water-tube type, but the simple shell type of still.

Another indication of the difference between the two arts is that for the purposes of carrying out the cracking away from the bulk supply, the structure which Clark showed in his patent and the structure which is actually used are clearly equivalent. They involve the same essential features, whereas for steam generation they are far from being equivalent; that is, circulating by pump through a long coil would not be a satisfactory method of generating steam.

The Master: You would not consider, would you, that the operation of making steam was in the same art as the operation of distilling petroleum?

The Witness: No, I certainly should not.

The Master: Then if you borrowed apparatus from some art which employed heat to operate upon a liquid, there would still be the element of invention for applying it to the new art, would there not?

The Witness: Especially when the function is entirely different—the function or result.

Q. So that if we recur to the Master's question as to the use of a tubular boiler for distilling oil under pressure, the only reference that mentions such a thing says not to use it?

A. Yes.

The Master: Of course, DeClerq comes closer, because he is operating here upon—presumably his tar is a hydrocarbon product.

The Witness: Mainly.

[fol. 834½] The Master: Thereby he is getting into the art.

The Witness: Yes.

Q. Those two inventions which you have named, the Humphreys false bottoms and the Clark tube still, conclude that group of inventions in the direct development of the Burton process, which you said had to do with minimizing the coke difficulty; that is correct, is it not?

A. Yes.

Q. Now, will you state what was the first improvement in the next group of inventions which you alluded to, in the improvement of the Burton process—that is, the improvement in the handling of the heavy portion of the distillate which the Burton process produces?

A. The first improvement in this class was an extremely important one, that of Humphreys, described in United States Patent 1,119,700, the invention being practically contemporaneously with that of Burton and used in one form or another on all the commercial forms of the Burton still.

Q. That patent that you identified by number is the one that we commonly refer to as the runback patent, is it not?

A. Yes.

[fol. 835] The Master: There are some observations on page 1631 of the exhibit book, in Exhibit No. 130 referred to,

DeClerq's patent, that seem to me to have an application. "With the structure of the present apparatus"—the second paragraph, do you see it?

The Witness: Yes.

The Master: After referring to the elimination of fire hazards.

The Witness: Yes.

The Master: And the principles of the process and structures "Consisting mainly in that a non-combustible heat carrier, for instance, steam or hot water, was introduced between the tar and fire", and the disadvantages. Then he says, "with the structure of the present apparatus the procedure is to separate the fire and the tar mass to be distilled so that they never come into contact, and therefore in case of a fire, no matter from what cause, the large quantity of tar which is being distilled at the time can never take fire."

Do you understand that that is simply separating his drum from his coils?

The Witness: I think that is what he refers to.

The Master: And in that respect he was doing just exactly what Clark was doing?

The Witness: In that respect as a matter of apparatus, yes.

The Master: Yes.

The Witness: As a matter of apparatus, the two apparatuses are rather similar, save for the location of the tar drawoff in that——

The Master: But if the apparatus had been shown prior to Clark, the process would have been disclosed, would it not?

The Witness: No. As I say, the apparatus for example, was not far different from that in water tube boilers. There are many processes——

The Master: If the apparatus applied to distilling hydrocarbons had been shown, the process would have been disclosed, would it not?

The Witness: I should not say so. Take, for example, the Burton still. A Burton still is as old as can be, but Burton carried out a new process in that old type of apparatus. Clark is carrying out a new process in a type of apparatus partially new, but the main invention is in the process that

[fol. 836] is carried out, not in the apparatus that is employed. In other words, Clark is not an apparatus patent, it is a process patent—a pressure cracking process involving certain distinct features.

The Master: But suppose some other man had patented the apparatus, could Clark come in and claim the process?

The Witness: A new process?

The Master: As a new process?

The Witness: Certainly, according to my understanding.

The Master: The operation of that combination which it was designed to accomplish, as a process?

Mr. Wiles: May I answer that? I think that is a legal question more than an expert question.

The Master: Of course it is, but I am trying to get at whether that occurs here—whether the facts occur which raise the question.

Mr. Wiles: Certainly. He could not repatent the disclosed use of that for the disclosed purpose, because the disclosure of the apparatus would then have disclosed the process. In other words, Clark could not possibly patent the use of DeClerq's apparatus for distilling tar at atmospheric pressure, because that would have been completely disclosed by DeClerq.

But if Clark takes DeClerq's apparatus, uses it under pressure, uses it on a different oil and not a tar, and thereby brings out certain properties which were perhaps always latent, but never disclosed, in that apparatus, then he can have a patent on that particular new use of the old apparatus.

The Master: All the distinction that is made there is the introduction of pressures.

Mr. Wiles: No, we are using temperature; DeClerq ran at noncracking temperatures. Clark runs at cracking temperatures. In other words, Clark discovers this particular old still which never became of the slightest importance as an atmospheric pressure still, but is the most useful cracking still when made strong enough and run at cracking temperatures. Now, that is a real patentable discovery.

The Master: That is a question of argument later on.

Mr. Wiles: Yes, of course. I may say that this question of the use of old apparatus and new process is, of course, in a good many cases like Carnegie against Cambria that

was mentioned yesterday. There the apparatus was a simply a big ladle; a blast furnace ladle.

[fol. 837] Mr. Wiles: The Supreme Court there says that the fact that the apparatus existed with which the process could be carried out hasn't anything to do with it. It must have been actually carried out or disclosed or so clearly indicated that one in the art would see it. The fact that the apparatus was there does not have anything to do with the process case. It is a question whether using it in this particular way to get these particular results is sufficiently disclosed in the art.

Mr. Rauber: That case is quite a ways from the facts in the present case.

Mr. Wiles: That is a question of fact. The utility is here and it is shown. It is a clearly different utility when you do not go to a cracking temperature from the case when you do.

The Master: I am sorry I started the argument but I thought it was best to get some impressions at the time.

Mr. Wiles: I think it is very much better to know just what we are driving at. I think it probably did clarify the Court's mind as to just what we are trying to show.

The Witness: In that connection, I think I should point out again, that this apparatus cannot be used successfully for Clark's invention, even if you add pressure, and even if you add the cracking temperature, because of the lack of a proper tar drawoff and the settling of coke in the chamber, which Clark mentions, and shows the provision for.

The Master: In Potts against Creager they were operating on a different material and that of itself made a distinction in the adaptation of the apparatus.

Mr. Wiles: Surely, but it was very, very slight.

The Master: I believe one of the illustrations of that was adapting a coffee pot for a glue pot.

Mr. Wiles: Even in mechanical cases the very slightest mechanical improvement to give some new function is novel.

The Master: The question of the adaptation of the old apparatus for the new processes.

Mr. Wiles: Surely.

The Master: Then comes in whether the old apparatus was used in the same art.

Mr. Wiles: Yes.

The Master: And the extent to which adaptation had to go, whether there was any exercise of the inventive faculty in it.

Mr. Wiles: That is when you are dealing with mechanical cases; when you are dealing with process cases you have quite a different rule because there you are dealing with steps, not with structures. Are the steps inventive as distinguished from the physical adaptation? Sometimes in process cases no adaptation at all is required; the exact old machine will do the new work.

The Court: Yes, if it is only one step in the process.

Mr. Wiles: Yes.

The Master: But if it is the entire process, why, the question is then whether there has been any new step at all in the process.

Mr. Wiles: At all; quite so. And here, we contend, the steps are those which are involved in turning to cracking and cracking temperatures, cracking pressures and so on.

Q. What was the first improvement in the practical art of handling the heavy part of the distillate which the Burton process produces.

A. The first improvement was a very important one, that of Humphreys, described in his patent No. 1,119,700 and known as the runback patent, the invention being practically contemporaneous with that of Burton, and embodied in all the commercial installations of the Burton process.

Q. Now, tell us what are the essential features of this runback patent?

A. The process disclosed by Humphreys consisted essentially in passing all the vapors generated by a large body of oil, held under pressure and at a cracking temperature, through an elongated passageway where all of the vapors are cooled and all of the insufficiently cracked material is condensed and returned to the still, counter-current to the hot vapors and through this elongated passageway. This returning the condensate through a long passageway counter-current to the hot vapors is important because it not only reheats the liquid to nearly the still temperature but it drives out practically all the gasoline which might have been [fol. 838] condensed and started back towards the still, so that the gasoline does not re-enter the still.

Q. In cracking without pressure was it common to return some of the heavy ends to the still for further cracking?

A. Yes, it was frequently employed in the prior art of atmospheric distillation. That is, the return of the heavy ends.

Q. That is in what we have commonly called the coking process, such as was described by Mr. Gifford?

A. That was frequently used in the coking process.

Q. What was the general theory back of this practice of returning in the atmospheric art?

A. For the most part it was this old dropping back theory of cracking to which I have referred, and to the discussion of which in the prior art references I have already called attention.

Q. Now, was it, to your knowledge, ever the practice to return to these stills substantially everything excepting a gasoline-like distillate?

A. No, it was not; nothing even approaching it. In fact, in my opinion, it would be entirely impracticable to attempt to keep in such a still any considerable part of the kerosene or even the light gas oil, because if you try to keep such material in an atmospheric still, the temperature in the still becomes limited by the low-boiling points of these materials, and therefore, it can never rise to a cracking temperature under those conditions.

The return of substantially everything heavier than gasoline was never contemplated and would be impracticable for the reasons specified in an atmospheric pressure coking still.

Q. Was the re-cracking of heavy parts of the distillate known in other types of cracking processes prior to Humphreys?

A. Yes, it was known in the tube cracking art and the flash cracking art.

Q. How about the bulk pressure distillation processes?

A. It was known to a very limited extent. Returning to the still a very small amount of the heaviest parts of the distillate was known in bulk pressure distillation.

Q. Now, will you please discuss first the problem involved in re-cracking the heavy distillate in the tube and flash-cracking processes?

A. Re-cracking of the heavy distillate in the tube and [fol. 839] flash-cracking processes involved no real problems at all. As I have already pointed out, the boiling point of the stock does not make any particular difference

in these processes. It can all be put through and heated to whatever temperature you want, and whether it vaporizes at one point or another point in the coil is, as a rule, not a matter of any moment.

In the flash-cracking processes, in particular, everything evaporates regardless of its boiling point, so it is simply a matter of handling it exactly as you would handle a fresh stock and involves none of these special problems which come in in trying to handle it in bulk pressure distillation processes.

Q. Now, will you enumerate the prior art patents, prior art tube and flashcracking patents which do re-crack some of the heavy distillate?

A. Noad, Testelin and Rennard. That is all I recall for the moment.

Q. How about Lang?

A. John Lang?

Q. The Boston Lang—was that John?

A. I don't think so.

Q. It was in the Boston Lang anyway?

A. Yes, it is L-a-n-g.

Q. Now, I want an enumeration of these prior art processes, which fall into the group where you started this trouble did not exist—that is, the tube processes—and you mentioned Noad and Lang—do you recall any others?

A. Testelin & Rennard, Oglobin, Schuchow & Gavrilow.

The Master: Those were all mentioned by Dr. McKee, were they not?

The Witness: Yes, they were all mentioned by Dr. McKee.

Q. And how about Bresson—"Exhibit No. 157"?

A. Yes, Bresson appears to re-run part of his distillate.

Q. So that he would go into that group, too?

A. Yes, sir.

Q. Now, what bearing on the applicability, or the inapplicability of this tube cracking process, to this Humphreys problem, has the fact that they do not require, as you have stated, any particular correlation of temperature and pressure?

A. The major problem which Humphreys had was the effect of his run-back on the proper correlation of temperature and pressure in a bulk pressure distillation proc-

ess. It has quite a profound influence on that, and since [fol. 840] the proper correlation of temperature and pressure, and, indeed, the whole question of pressure—are very minor matters in tube cracking processes, the effect of this light stock, different from the original charging stock, on these factors does not enter into the tube cracking processes, at all—really, it constitutes the whole problem in bulk pressure distillation, as I shall shortly point out.

Q. Now, tell us about the special problems, if any, which arise in connection with the re-cracking of heavy distillates in the bulk pressure distillation processes?

A. The attempt to re-crack substantially all the distillates heavier than gasoline, in a bulk pressure distillation process, involves several very serious problems. Suppose we start with a still charged with gas oil, and we heat it up to, say 750 degrees, and at 75 or 95 pounds pressure we will start to crack at quite a rapid rate, at that temperature, with fresh stock in the still. The distillate, however, coming off, under those conditions, will be quite poor in quality—that is, it will contain not only the gasoline, but, in general, two or three times as much kerosene as gasoline.

Now, there are different possible ways to handle that problem. In the first place, we can lower the still temperature and get a different correlation of temperature and pressure, which will tend to keep the kerosene in the still and not have it distill out under that pressure. If we lower the temperature, say twenty-five degrees, which would probably be necessary to make anything like the desired improvement, that will cut our cracking rate to less than half of what it would be at 750 degrees.

Furthermore, if we cool it down enough to leave the kerosene in the still, we will inevitably have a considerable amount of gasoline left in the still, as well.

I pointed out, in stressing the importance of correlation of temperature and pressure, the desirability of selecting a pressure which would keep the kerosene, so far as possible, in the still, and allow the gasoline so far as possible, to escape.

But the nature of the simple distillation as it is called—and by simple, I mean there is no fractionating, or rectifying action taking place—is such that you cannot make, even though you choose your best possible pressure—you cannot make a sharp separation, which will separate two things

as close together as gasoline and kerosene. That is, either you must take out a great deal of kerosene, or you must [fol. 841] leave in quite a bit of gasoline, and, of course, leaving in the gasoline means re-cracking of the gasoline, making more gas and decreasing the yield of gasoline. So that that is quite undesirable.

If we decide, then, for these reasons, that we do not want to lower the still temperature to keep the kerosene in the still, but we are going to operate at a high temperature and get the high cracking rate, and avoid re-cracking, but get a poorer distillate, then the question comes up, what shall we do with the undesired part of the distillate?

If we take that off to another still and distill out the gasoline, and send back the kerosene to a separate pressure still, and try to crack it, we will find that it cannot be cracked commercially at the pressures which were reasonably feasible in 1912—that is, anything like the pressures with which Burton was working.

As I pointed out before, it can be sent back in a tube cracking process, and not make any trouble, at all. But it cannot be sent back in the other process. In fact, even today we find that in order to crack material of that general character, or somewhat heavier than that type of material—that is, the heavy ends of the kerosene—we have to go to 325 pounds pressure, to get a satisfactory cracking rate, and that would not have been available in those days.

The Master: Do you mean to say that it would not have been available because you hadn't the apparatus that would stand the pressure?

The Witness: The art of boiler making and still making had not progressed, in 1914, to the point where you could make a large scale apparatus capable of standing such pressures and temperatures with safety. Today, as I shall show later, those difficulties have been overcome.

The Master: Mechanically?

The Witness: Mechanically. That was the problem that Humphreys had to solve.

Now, suppose one had the idea, instead of taking the entire low grade distillate out of the still, and redistilling, or trying to crack the kerosene separately, to try to subject the vapors to pressure condensation and return then to the still? Again, you are up against the problem that if you condense out, in ordinary condensation, all of the kerosene, you in-

evitably condense with it a considerable amount of gasoline, and when you put that back into the still you waste a lot of heat in vaporizing the gasoline and in heating the [fol. 842] kerosene up to distilling temperature, and are also likely, to a certain extent, to re-crack the gasoline, which is just what you are trying to avoid.

So that, while it is perfectly possible to de reflux condensation of a little bit of the heavy ends—say 5 per cent of heavy ends—leaving the still, and send them back to the still, without having any marked effect on the boiling point and heat consumption—if you are trying to do what Humphreys was trying to do—return eighty to ninety per cent—you have an entirely different problem, and a very difficult problem, to put that much material back into the still, and yet maintain rapid cracking and satisfactory operation.

Q. Could such an operation as you have described, in returning eighty or ninety per cent, be accomplished in an ordinary still dome?

A. No. At any reasonably running rate, in the ordinary still dome, would generally not return over one per cent. You must have the refluxing surface, as Humphreys discloses—and is actually in use in practice—a refluxing surface greater in area than the heating surface of the still.

It is a tremendous problem, beyond the merely knocking back a little of the heavy ends, but can be accomplished in a certain way.

Q. Describe the method that Humphreys adopted to overcome those difficulties.

A. Humphreys' plan was just this: To take out these entire low grade vapors from the still, to such an extent that you will have no considerable amount of gasoline in the oil in the still—which is the first essential. That also insures a fairly high cracking temperature. Then to take all the vapors through an elongated fractionating device, and by the fractionating device make a much sharper separation. By a gradual gradation of temperature—in other words, such as we get in a bubble tower—by passing through this elongated passageway, where the temperature gradually drops as we go up—by that means we can cool all the vapors, to a certain extent, and by pressure we can condense practically everything except the desired product which escapes from the top of the run-back, and is condensed separately. The material, however, which is con-

densed in the upper part of the run-back, the cooler part of the run-back contains considerable gasoline, and if that is put directly back into the still it does lower the still temperature, and gets gasoline back into the still, which we want to avoid.

[fol. 843] But if we allow that high condensate to run back through this same elongated passageway, counter-current to the rising stream of vapors, it will become heated up, practically to still temperature, and we will distill off the undesirable light ends, which we do not want to go back into the still, and put back into the still only the heavy condensate, which we will then get a chance to crack further.

The Master: You used the word "undesirable" in an equivocal sense, in that answer. What you called undesirable was the really desirable thing?

Mr. Wiles: Exactly.

Q. You mean that the light ends are undesirable back in the still, but that they are desirable over in the condensate?

A. Yes.

The essential steps in the Humphreys' process are the cooling of all the vapors; (that is necessary to produce this fractionation) in an elongated passageway, (which is also an essential step in the fractionating action) condensing of a far, far larger proportion than the prior art did—and then returning that condensate, counter-current to the vapor, so that counter-current to the vapors, so that it is heated nearly to still temperature, and stripped free from the gasoline which was condensed in the cooler part of the apparatus.

Q. Now, does the Humphreys run-back, on a pressure still, cooperate, in any important way, with the process of condensation under pressure, to produce a new and useful result?

A. Yes, it does. As I pointed out in connection with the Dewar & Redwood patent, it is necessary, when there is a still and a condenser in free communication, and the liquid level is maintained in the drum, that we produce enough gas to saturate the distillate at the operating pressure.

In other words, our gas to distillate ratio, must be large enough. As I also pointed out, when you are making kerosene, or making a very low grade distillate, you generally don't have enough gas to saturate the distillate, and you

are in trouble to maintain the pressure. Now, that difficulty can be overcome, conceivably, in one of three ways. You can add gas by pumping it in from the outside, as Dewar and Redwood did—and that, of course, increases the ratio of gas to distillate, by increasing the gas. You can do [fol. 844] that same thing in a process such as Lang has, by re-cracking the vapors as they come off, and so crack part of the distillate, and make more gas, as is characteristic of the tube process.

The Master: That would be at a loss?

The Witness: Yes, sir. Or by the Humphreys method you can decrease the amount of distillate that has to be saturated, by sending the bulk of it back to the still, so that the gas is sufficient to more than saturate the amount of distillate that is allowed to come over into the drum.

Q. Now, can you illustrate the practical importance of the runback on the maintenance of pressure in this type of process?

A. Yes. I tried an experiment in connection with the Dubbs case previously mentioned, of operating a laboratory pressure still with a runback to accomplish the ordinary purpose, and also with a vapor line which went directly from the still to the condenser.

The Master: Mr. Wilson, is the runback shown in your diagrammatic illustration, Exhibit No. 405——

The Witness: The original Humphreys' runback——

The Master (continued): Is that the runback of the Humphreys patent?

The Witness: No, the runback of the Humphreys patent is two very large and highly elongated vapor lines which were later increased and changed by the Moore invention of the radiator and by the Lewis and Cook bubble tower.

Mr. Wiles (handing drawing to Master): That has got the radiator added to it, this is it, plus the radiator (indicating).

The Master: I just want to distinguish because both are called runback and I just wanted to have in the record here what you were referring to as a runback.

Q. The runback in the Humphreys patent that you are now discussing is described as of a suitable size for an 8 by 20 still, is it not?

A. Yes.

Q. Which is smaller than you now commercially use?

The Master: Two pipes 12 inches in diameter, forty feet long.

Mr. Wiles: Yes.

A. That is smaller than we commercially use. For a tube still or a ten by thirty shell still the runbacks must of course be increased proportionately to accomplish the re- [fol. 845] sult which Humphreys clearly described, of cooling all the vapors and condensing all the undesired, insufficiently cracked material.

The Master: Then ratio and dimensions are of importance in this art?

The Witness: Yes.

Q. As these stills were enlarged you amplified the runback, among other things, by the radiator which is shown in Exhibit 401?

A. Yes.

Q. Now, will you continue with the description of the experiment that you said you made in connection with the Dubbs case where you ran the laboratory still with and without the runback? Describe the apparatus a little more fully, please.

A. The apparatus was a small process laboratory still, as we call it, holding about a fifty gallons charge of oil and we ran it disconnected from the high line; that is, closing the valve between the receiving drum on the small still and the high line, which was maintained at ninety-five pounds pressure, once we got the still up to the operating pressure. Of course, we had to put in gas at first to bring it to the operating pressure.

We found that when we were using the runback and refluxing a large proportion of the distillate to the still in the normal way, we were making much more than enough gas to saturate the distillate, as shown by the fact that to maintain the operating pressure we had to keep opening the valve to let excess gas escape and then we would close it until the pressure rose a few pounds in the receiving drum and then we would open the valve again and let that excess

escape, proving that we were making more than enough gas at that time to saturate the amount of distillate which came over.

Q. May I ask at this point whether that laboratory still that you used had a runback of the same proportions as those commercially used?

A. I would not say it was exactly the same proportions, but it re-fluxed to the same extent. That is, it permitted us to maintain the same still temperature and get the same quality of distillate, which shows that the runback is accomplishing the same purpose as the plant runbacks did, and by other tests we know that it was refluxing approximately the same amount as the plant stills did.

The Master: You say that you would not say it was the same ratio?

[fol. 846] The Witness: No, the reason it is not the same ratio is that the lower part of this runback was lagged; as this was a later form of runback, more like the Lewis and Cook bubble towers, in which we lagged the lower part and had most of the condensation taking place near the top, which is a preferable method of operation——

Q. What I was getting at was whether it was a runback designed to return the same reflux ratio?

A. Yes. That is what I said.

Q. In that respect, therefore, it was a fair duplication of your regular plant conditions?

A. It accomplished the purpose which Humphreys disclosed in a slightly different manner, more like what Lewis and Cook disclosed later.

Q. Now, will you continue, please.

A. We then closed the valve in the line going to the runback system and opened the short direct line between the still and the condenser, so that all the vapors which had been going up into the runback and there condensing and falling back, now went over by the bypass line and back to the condenser, so that we had no refluxing action.

The amount of distillate was, of course, veyr much increased and almost immediately the pressure began to drop, so that in order to maintain the pressure in the drum we had to let gas in from the high line to bring it back to the ninety-five pounds. Then we closed the valve again and we

found the pressure dropped again, due to the fact that we had more distillate than we could saturate with the amount of gas which we were producing.

Q. So that, briefly, the runback is very important in enabling you to maintain pressure when you run the way you are commercially running?

A. Yes.

Q. Now, what bulk pressure distillation processes were cited by plaintiff's expert against the Humphreys runback patent?

A. The Engler modification of Riebeck and the Laing, the Dewar and Redwood, the Pielsticker and the Barbet processes; the latter process described in Exhibits 141 and 136.

Q. Now, do any of these processes disclose the invention of Humphreys, the essential features which you have stated here?

[fol. 847] A. No, none of them even approach the essential features of Humphreys' invention.

Q. Now, what does Engler do in plaintiff's exhibit No. 99?

A. Engler merely takes part in the distillates which he produces in a first cracking of fish oil and puts them back into the still for a second cracking. That not only is entirely different from what Humphreys did, but, as I pointed out, that method of operation won't work as a solution of Humphreys' problem. That is, we cannot take the heavy part of our distillate that is produced and put it back into another still and crack it at anything like ninety-five pounds pressure at a commercial rate.

Q. You do not make the last statement quite clear to me, Mr. Wilson. When you said that Engler put it back again, did you mean that he put it back in the same run or put it into another still at another time for another run?

A. Into another still at another time for another run. He had no runback in any sense which we would use it or that could be described as a runback at all.

Q. By runback you mean a return of the oil to the body from which it came?

A. Yes.

Q. Now, please compare the disclosures of Laing with that of Humphreys?

A. John Laing in his sectional form of still had a peculiar device which I have mentioned before, which took the vapors

from the first or hottest section of the still, condensed them and returned them relatively cold to the next section of the still, then took the vapors given off from that and sent them up into a second condenser and down into the third compartment and repeated the operation between the third and the fourth compartments.

As I pointed out, this process of Laing was simply carrying to its logical conclusion the erroneous ideas as to the great value of dropping back relatively cold oil into a hot body of oil, and that all he really accomplished was the waste of a terrific amount of heat and also the reduction of still temperature, so that he got very little cracking beyond the first compartment, if he got any appreciable condensing action.

In addition to that, Laing is very clearly distinguished from Humphreys in that he did not return his condensate to the body of oil from which it came; that he did not return his condensate counter-current to the ascending vapors so as to strip out the gasoline, but condensed everything he could and put it all back into the still.

Furthermore, he failed to have any cooperation between the runback action and condensation under pressure because his final condensation was not under pressure.

Q. I wish you would very briefly compare Pielsticker with Humphreys, but only in a general way, reserving your detailed consideration for a later point.

A. Pielsticker got the ordinary slight amount of condensation of heavy ends, a matter of a per cent or two, in all probability, which was fairly common in all types of stills, e-ther with an ordinary dome or with a somewhat enlarged and modified dome, but had nothing even remotely approaching Humphreys' runback, either in the amount or in the method in which it was handled.

[fol. 848] Barbet in both exhibits 136 and 141 has the process which I described in connection with the large charts yesterday, and the only basis on which that was cited, both against the rerunning for the return of distillate from the pressure tar and against the Humphreys patent, was on the erroneous assumption of plaintiff's expert that it was possible for some undisclosed and unmentioned gas oil cut to work its way around and back into the pressure still, which I clearly showed yesterday was not possible at all under the operations which Barbet disclosed. So that

that is not in any sense, regardless of the other distinctions, a reference to the Humphreys' invention.

Dewar & Redwood's disclosure of a runback is a very strange one; thus they start out by saying in line 31 of the first page of their patent, page 1359 of the book of exhibits:

"It has also been proposed to arrange the still boiler with its upper part cooled so that the best volatile portions of the vapor may become more or less condensed and fall back into the hot liquid below, this mode of operation being commonly termed 'cracking'. Both of these methods are objectionable, the former on account of the irregularity of the distillation, and the latter on account of the waste of heat in conducting the cracking process and the slowness and insufficiency of the results."

However, in spite of this condemnation of this popular theory of cracking, they go on to say rather half-heartedly in line 68 of the same page,

"We may also arrange the still head or upper part of the boiler or retort so as to operate according to the cracking method above referred to."

This half-hearted attitude toward the runback is fu-ther indicated in their next reference, lines 55 to 59 of page 2 of the patent, where they say,

"During the distillation such of the vapor as may be condensed in the still head C2 or such liquid as may prime up into the still head flows back into the body of liquid in the retort."

and they add, line 65:

"If necessary, means of cooling may be applied to the still head C2."

The terms "priming" or "entrainment" which are common in all sorts of distillation arts, refer to the carrying along of drops of foam or liquid with the vapor, due to its too rapid evolution, and of course that prevents the effectual separation of vapor and liquid, so priming or entrainment are always very undesirable. That is one of the purposes of the use of the ordinary still head, to prevent priming, rather than to have any particular condensing action.

Q. The liquid which primes is undistilled stuff that has not been boiled at all, but is just physically swept along; that is corect, isn't it?

[fol. 849] A. Yes, and since the object of distillation is generally to separate the vapors from the residual liquid, it is very undesirable, and you want to knock back any primed liquid into the still where it belongs and keep it out of the condenser.

To my mind the net result of this disclosure of Dewar & Redwood is merely to tell to anyone interested that they do not take any particular stock in this runback theory of cracking, but that with their apparatus there are means there so you can try it out if you want to and see whether it does any good or not.

Their attitude is certainly not that of commendation or recommendation of the runback principle. The disclosure certainly is in very marked contrast to that of Humphreys, who emphasizes strongly the partial cooling of all the vapors and the condensation of everything except the desired distillate. There is nothing in Dewar & Redwood which remotely approaches such a disclosure.

In the normal action of the Dewar & Redwood device what would happen is, as they indicate, the stopping of any tendency toward priming up, (carrying liquid drops along with the vapors) and a small amount of condensation, similar to the condensation which takes place in any ordinary still head, and of course a condensation of only comparatively heavy ends. If you adopt their suggestion as to the possibility of using some undisclosed means for getting greater cooling on the still head, such as, for example, the spraying of water on it, you will note the rather peculiar form of this still head on page 1358, probably designed to stop entrainment more than anything else. What would happen if excessive cooling means were applied to make any considerable condensation would be a rather complete cooling of part of the vapors in the dead end with a relatively hot core of vapors passing straight through to the vapor line, an action entirely different from what you get if you cool all the vapors in a greatly elongated vapor line, so that they are uniformly at the same temperature.

Furthermore, if any considerable amount of materials are condensed in these cool ends of the device they would necessarily contain a considerable quantity of gasoline, and in re-

turning through this very short neck to the still they would get nothing like the very efficient heating and stripping action which Humphreys gets in his forty-foot vapor lines, which have the specific and very great advantage of heating [fol. 850] and effectively stripping the gasoline from the condensate. Of course, in any case, what Dewar & Redwood would want to return to the still would be material heavier than kerosene. He might return some kerosene inadvertently, but he was working for kerosene as the desired product, and his product, which he desired over in the final receiving drum, is the principal material which Humphreys is trying to condense and send back to the still.

The Master: The question is whether he suggests the idea even though operating in a less efficient manner.

The Witness: To summarize, what Dewar & Redwood evidently contemplated was the optional return, if you desired, of a small amount of heavy ends, with the suggestion that you could increase this return if you wanted to; but the means which they showed for accomplishing that would neither give the close fractionation of the distillate which I have shown is very important to Humphreys, the heating and stripping action to remove the gasoline or the very light ends from the return stream, and, furthermore, he was not dealing with the same distillate at all, as Humphreys was concerned about, and therefore he did not have the same problem.

Q. As I understand the description which you have made to the Master, if you do use this additional means of cooling you are going to overcool a part of the vapors and you hardly cool the rest of them at all?

A. Yes, that is essentially what happens. You get more of a waste of heat than anything else.

Q. Whereas, Humphreys cools all the vapors to just the right point?

A. Which gives, of course, a very different result.

The Master: In your judgment did Dewar & Redwood fully grasp the essential idea of the runback?

The Witness: No, I do not think they even had a glimmering of any such thing as Humphreys was trying to do. I think their attitude was this: "' Now there are a lot of people who recommend this dropping back action, and there may be something to it.

The Master: That is, the old misconception of what happened by dropping back.

The Witness: Yes, they got to the place where they did not think that was right, but they hated to take a flat-footed position against it, and they said in effect if you want to try it, why you can do it with this type of still. We have to have [fol. 851] a still head anyway for knocking back the entrainment or preventing this priming up, and if you want to apply additional cooling means that is possible. But the type of dropping back they would have gotten, of relatively cold liquid in the still, was the prior art type, and not the careful fractionating in an elongated passageway and the careful stripping, which is vital to the return of any such product as Humphreys wanted to return to his still, and yet maintain the pressure and temperature.

Q. At a later point, when we get to Pielsticker, we will have to go into this question of firing and cooling surface, but right at this point, if it has not been made perfectly clear, I want you to tell the Master how much of the vapors you do return through one of these Humphreys runbacks today. I do not know whether that has been stated or not.

A. Yes, I have already stated we returned eighty to ninety per cent of the vapors leaving the still. It has been my pleasure to determine that accurately as one of my first problems with the company. There were a good many guesses as to just what was returned, and it was rather hard to be sure. But it is the general practice on our stills today, to return from eighty to ninety per cent of the vapors which leave the still.

Q. If you take the Humphreys figures given in the patent, you find the cooling surface there is actually greater than the firing surface, do you not?

A. That is correct, and that same ratio is preserved in all the commercial installations so far as I have been able to find.

Q. By that I mean the cooling surface in the runback. The runback has a bigger area or surface than the fired surface of the still. Dewar & Redwood is very much smaller than that, as indicated in the drawings, is it not?

A. Yes, and yet they are probably larger than any other of the prior art devices. In fact I am pretty sure they are larger.

The Master: Your position is that they never grasped the essential theory of Humphreys, and they provided no means for either partially or effectively carrying it out?

The Witness: That is right.

Q. Still leaving Pielsticker for later, because we will have to take that up more in detail, what was the next development in his direcion of handling he runback?

A. The next development in this line was the Moore radiator described in United States Patent 1,130,318, which I have here, the Moore radiator being essentially a more convenient disposition of the large amount of cooling surface necessitated by the use of larger stills and somewhat greater heating surface. To continue indefinitely these twin twelve-inch vapor lines, to accommodate larger stills and faster firing rates and things of that kind, would of course make them entirely too long for practice. So Moore suggested the disposal of the additional cooling surface at the end of these vapor lines in the forms of harps or radiators shown in the drawing of his patent.

Q. And those are the same ones which are shown in Exhibit No. 401, are they not?

A. Yes, in the drawing of the Burton still.

[fol. 852] Q. What was the next development in handling the reflux problems?

A. The next development was that of Clark, described in his patent No. 1,388,514, of the process which is commonly called the runback. It was soon indicated, that with the improved methods to taking care of the carbon inherent in the Clark type of tube still, it was possible to feed the stills to a very considerable extent.

The Master: What patent is this?

The Witness: This is Patent No. 1,388,514. Clark discloses in this patent the very useful expedient of introducing the feed, not directly into the still or into the tubes, but into the runback near the top. This method of introducing the feed has three important advantages. In the first place it drives off any water which may be present in the crude and which it is particularly desired not to introduce into a pressure still because of the sudden flash, almost explosion, which takes place when water is introduced into hot oil in any considerable quantity.

Of course, this water in the feed is purely that which is there accidently. Every attempt is made to keep the feed dry, but there are always opportunities for the water to get in. In the second place, this method of introduction it drives off any light ends which are too light to handle and crack satisfactorily in the still, and, third, and most important from the standpoint of economy, the feed is heated by the rising vapors with what would otherwise be waste heat—that is, reradiated to the air, as in the ordinary Humphreys form of runback. That heat is used in part to heat up the runback, so that only a smaller amount of heat needs to be radiated to the atmosphere in order to condense the heavier portion of the vapors leaving the still.

The Master: You mean Humphreys' runback or Clark's runback?

The Witness: Humphreys' runback.

Q. Mr. Wilson, I notice you said "water present in the crude." Did you mean to use that expression?

A. No, I meant to say feed instead of crude. Of course, the feed to these stills is the same stock that is used in all the operations—namely gas oil and similar materials.

Mr. Wiles: This Clark patent is offered as Defendants' Exhibit 407.

(The said patent so offered, was admitted and read in evidence, was marked "Defendants' Exhibit 407," and the same is herewith returned.)

[fol. 853] Q. You have just considered, Mr. Wilson, two patents, the Moore radiator and the Clark feeding the runback patent, which were not offered by the plaintiff or considered by plaintiff's expert. How extensively are those employed by the defendant, the Standard of Indiana and the various licensees who use the Burton process?

A. I believe one or the other of those devices is used on all the stills of the Indiana Company with the possible exception of a few at Wood River. I am not certain whether there is an exception or not. The Moore radiator is also used by a very considerable number of licensees, and the system of feeding the runback is, I believe, used on all of the tube type of cracking stills, though seldom if ever used on the shell type of cracking stills.

The Master: You mean all of the tube type cracking stills of licensees?

The Witness: Of licensees, as well as of Indiana.

Q. Mr. Wilson, in going over my notes here I am afraid I omitted a question this morning while we were on the Humphreys' runback. You brought out the point that there was an important cooperation between the runback and the pressure system—that is, the possibility of maintaining pressure.

A. In the condenser, do you mean?

Q. Yes, pressure in the condenser. I do not recall asking you whether any of the prior art processes had this function, even apart from the question of whether they had the Humphreys' runback, broadly.

A. No, none of them had this function, even apart from the question of whether they had the runback, because the only device showing any type of runback, however small, which condensed under pressure, was Dewar & Redwood, and Dewar & Redwood provided their necessary gas distillate ratio by quite a different means, namely, pumping in extra gas.

Q. Following the Clark patent on feeding the runback, which you have just discussed before the last question, what was the next development along this line of handling runback?

A. The next development was the Lewis and Cook bubble tower.

[fol. 854] The bubble tower is essentially an enlargement and improvement in several directions of the Humphreys runback, and is essentially an improved substitute for the original Humphreys runback.

The Master: Is this bubble tower different in its construction from the other bubble towers?

The Witness: Mainly in being much stronger to stand the pressure, but the general features of its construction are not novel.

The Master: Its principle of operation was old?

The Witness: Its principle of operation as a pure fractionating device, yes. It has certain additional functions in this type of apparatus.

The Master: Its connections and its place in the process that gave it patentability.

Mr. Wiles: Yes.

The Witness: As a mere piece of apparatus, it involves no novel feature.

The Master: Yes.

Q. And that is illustrated in its connection in Exhibit 405, which your produced this morning in connection with your discussion of the tube still?

A. Yes, it is.

Q. Just tell what this patent adds to the disclosure of Humphreys?

A. In addition to accomplishing even more efficient fractionating of the distillate, and even more efficient stripping of the feed returned to the still, in that way being an improvement to some extent over Humphreys, it had two unexpected and quite important properties due to the fact that as Lewis and Cook specifically pointed out the vapors are passed through successive pools of liquid held at successively lower temperatures. These two unexpected advantages are, first, that there appears to be at this intermediate temperature slightly below a cracking temperature and yet far above room temperature, a change which takes place, which in some way greatly reduces the amount of gum-forming constituents which are present in the distillate. A distillate taken from these tube bubble tower stills can be very much more readily refined to make a stable [fol. 855] and substantially gum free product than the distillate which passes through Humphrey's device, and our evidence indicates quite strongly that that is due to the passage through successive pools of liquid, and giving it time to react before the distillate comes out. The other advantage is that these successive pools of liquid at gradually lower temperature and gradually lighter in composition acts a- a stabilizing influence on the operation of the still. There always tend to be minor fluctuations in the rate of distillation due to wind and firing and other things. With a Humphreys type of runback the amount of liquid held in there at any given time is so small that its stabilizing action is very small and minor fluctuations in the operation of the still are at once reflected by changes in

the composition of the distillate in the receiving house. But with bubble tower if heavier distillate or more distillate begins to come off it simply gradually pushes up the composition on each of the plates, and we have a storage supply of good quality light distillate which comes over if the change in the conditions is only temporary. Of course, if the change in conditions is permanent, eventually that poor distillate will get into the house, but it does act as a stabilizing mechanism for preventing minor fluctuations in still conditions being reflected in wide variations in the composition of the distillate.

I should add that even though it may be true as plaintiff's expert claims, that there are other types of devices, such as the filled towers, which are more efficient as fractionating media than a bubble tower, they do not have these other two functions because they lack the considerable quantity of liquid and the relatively long time that liquid is held in the tower.

Q. I notice, Mr. Wilson, that you used the expression "coming into the house". By that you mean, do you not, the receiving drum which is in the receiving house?

A. Yes.

Q. Now, what evidence have you that these results cannot be brought about by other types of towers, as plaintiff's expert indicated they could, and even better than Lewis and Cook carried them out?

A. One of the thoughts I had soon after I came with the Company was that it should be possible to build a fractionating tower which would be equally or more efficient than the Lewis and Cook bubble tower, and yet be cheaper and [fol. 856] not corrode away so rapidly as it tends to do with the action of the corrosive constituents in the vapors.

We therefore constructed a special form of filled tower and operated it for a period of months in comparison with similar stills with the ordinary type of tower, and we found to our sorrow that while we were able to get substantially the same fractionation, as reflected by the boiling point curve of the distillate, we could not get as good colors or as low gum, and the still operated so much less steadily that the stillmen complained about it being hard to run. They, of course, tried to maintain the distillate of a uniform quality and they found it more difficult with this type of tower, which had no pools of liquid in it.

Q. Was this experimental tower that you built a thoroughly efficient fractionating device?

A. Yes, it was, so far as the mere fractionation was concerned.

Q. Of the same kind of filled or packed tower of the general type that plaintiff's expert testified to?

A. Well, it was a special type. There is no general type of packed towers. There are many different types of packed towers.

The Master: My original question was whether the mechanical construction and principal of operation of this bubble tower of the Lewis and Cook patent is different from any antecedent bubble tower or fractionating tower.

The Witness: Not as a tower, no. It was not different essentially.

Q. Do you find any tower in the prior art where polymerization takes place in the pans?

A. I have never seen any indication of that action. It may, of course, take place in some other industry, I do not know.

The Master: The invention then lay in selection and adaptation and its position in the process?

The Witness: Which produced a new and unexpected result.

The Master: Yes.

Q. Now, do you agree with plaintiff's expert on page 445 of the record that essentially the same results could be obtained by passing the vapors into a bubble tower at atmospheric pressure?

A. No, I do not. As far as mere fractionation is con- [fol. 857] cerned, the results would probably be substantially as good, providing the tower were made enough larger in cross-section to take care of the higher velocity of the vapors after their pressure was released; but we have found experimentally, first, that the polymerization of the gum-forming constituents at the relatively lower temperatures corresponding to the lower boiling points of the reflux at atmospheric pressure, (of course, each of these pans is filled with liquid substantially at its boiling point), is by no means as effective as we obtain through the higher temperatures. In fact, by far the most effective

polymerizing action we have is in the still higher pressure stills such as the Holmes-Manley stills, operating at pressures from 300 to 400 or 500 pounds.

In the second place, to show that the results are by no means equivalent in the atmospheric pressure tower, when operating the tower at atmospheric pressure, reflux of a given composition cannot be returned at a temperature higher than its boiling point, and that is approximately 175 or 200 degrees lower at atmospheric pressure than it is under still pressure, so that all this tremendous volume of reflux must be heated by that extra amount by the still instead of picking it up from the vapors as they come from the still, which is a source of heat that costs nothing.

And, in the third place, this would involve pumping the material from the low pressure back in the high pressure and which is, by no means easy to accomplish on an oil which is substantially at its boiling point. The pumps tend to get vapor-bound and run away.

Q. Whereas, in the form shown in the patent it runs back by gravity?

A. Yes.

Q. But there would be some advantage as set forth in an affidavit in this file in operating at atmospheric pressure?

A. Yes, there would be some polymerizing action and quite efficient fractionating action takes place at atmospheric pressure.

Q. Now, I wish you would please compare the Lewis and Cook disclosure with the disclosure in the Barbet Exhibit 136, cited against it by plaintiff's expert.

A. In Barbet we have the only example I have been able to find of any cracked vapors going through a bubble tower.

As I pointed out, however, the character of the vapors is [fol. 858] entirely different, presumably being made from shale petroleum and consisting almost entirely of paraffin distillate and illuminating oil. Presumably the function of polymerizing the gum-forming constituents as in Lewis and Cook would be nearly or almost entirely absent.

Furthermore, the Barbet tower was not operated under pressure. If we take the broader aspects of the Lewis and Cook invention on the use of the tower, either with or without pressure, then Barbet is admittedly a pretty close reference, but it entirely lacks the great additional advantages gained by Lewis and Cook in their narrower aspects

of working under pressure, which means the return of reflux to the still preheated almost to still temperature, and the ability to run it back by gravity instead of pumping it back, and the much accelerated polymerizing action that takes place by keeping the tower at the elevated temperature corresponding to this pressure.

The Master: The advantages then are economy of operation, quantity and quality of production.

The Witness: Particularly the quality of the production and the element of greater safety in operation, too, of avoiding the necessity of having a pump forcing hot distillate back into the still.

Q. I would like, Mr. Wilson, to compare just a little bit more of this Barbet with what you have stated was the broader aspect. As I understand your testimony, Barbet's stock is crude oil, is that correct?

A. It is crude petroleum; the word "petroleum" being used presumably in the European sense of shale oil.

Q. So that the starting material certainly is not shown to be the same as the starting material in the Lewis and Cook patent?

A. (continued): The starting material, whether crude oil or shale oil, is certainly far different from the gas oil and similar materials.

Q. Now, judging by the character of Barbet's residuum, which he said was suitable for pressing, how did the two cracking operations compare, in kind?

A. There is almost no comparison between the two. Evidently Barbet's cracking was extremely light, and of the vapors going into his tower, it is doubtful—of course it is a pure estimate—it is doubtful if more than ten per cent were actually cracked vapors.

The main materials were paraffine distillate and tar, which was uncracked, or he could not possibly have pressed through paraffin presses, his residuum from the still. So what cracked vapors were there, were greatly diluted with distillates from the oil.

Q. Now, with a different starting material, and a very light crack, are you able to say whether the cracked vapors would sufficiently resemble Lewis & Cook's, so that the function would be the same in the bubble tower, or not?

A. I am not able to say definitely, but I should doubt very much that it was the same.

Q. Now, the only other reference which the plaintiff's expert cited against the use of a bubble tower, was Pielsticker, as described in the American and British patent, plaintiff's "Exhibits Nos. 133 and 134." Those patents were also cited against the Humphreys run-back, and against a number of other patents which you have not yet discussed.

In view of their importance and their involved nature, and variations in process, and in the apparatus, I want you to go through this Pielsticker disclosure in detail.

I will ask you, first, whether there is any essential difference between the British and the American patents?

[fol. 859] A. The two patents are essentially the same in disclosures. They are apparatus patents, giving a number of different alternate forms of apparatus, and a number of different alternate methods of operation. But they are essentially the same, except for differences in the lettering of the drawings, and in the fact that the American patent has two more drawings than the British patent, and, therefore, is probably preferable as a basis for discussion.

The Master: They are substantially the same?

The Witness: Yes, sir, they are substantially the same.

The Master: In their language and order of their statements?

The Witness: Yes, sir.

Q. Now, what was the Pielsticker's primary purpose in his invention?

A. Pielsticker's primary purpose in his invention is set forth on page 1670, referring to line 18 of the American patent:

"My invention relates to improvements in the distillation of hydrocarbons, or other oils, and liquids by means of which the operation of distillation is conducted in a continuous manner."

In other words, he is interested, especially, in carrying out distillation in a continuous manner more or less along the lines of a pipe still method of distillation, which is coming into increasing favor. He also mentions later on that

the same apparatus can be modified slightly in its operation, to carry on cracking.

The Master: What is the date of his application?

The Witness: 1892—no, the application was 1890, and the patent was issued in 1892.

He also says, with regard to his object, on page 1672:

"My apparatus may also be used for the distillation of other liquids besides hydrocarbons. For instance, for fats and oils in the separation of fatty acids and glycerine. Also for the continuous production of distilled water from sea water, or the concentration of brine and alkaline solutions and the concentration of sugar solutions."

In other words, he is interested in developing a continuous form of apparatus for distillation, and he directs himself to describing the apparatus in great detail, and the various uses to which it may be put.

The Master: Mostly simple distillations?

The Witness: Very largely simple distillations—well, he has both kinds of operations, which we will have to take up in detail.

Q. Now what is the first element of Pielsticker's apparatus and what is his reason for its construction?

A. The first element of Pielsticker's apparatus is that [fol. 860] shown in Fig. 2 on page 1666, namely, his pre-heater, which is of a rather involved construction, in that is consists of holes drilled out of a solid billet of steel, and has a special method of closing up the ends to form return bends, which bends can be removed for easy cleaning.

This element in the apparatus is used in all the alternate forms of operations which he suggests. Its function is to prevent local overheating and burning out of the apparatus, and to make it possible to clean out the tubes, if that be necessary.

Q. And is this pre-heater part of the apparatus stated to be an improvement on anything else?

A. It is stated to be an improvement on the type of pre-heater which he used in a prior application,—British patent No. 1706, of 1882.

Q. And the improvement was in this direction of minimizing the burning out of tubes?

A. Yes.

Q. Now, does the formation of carbon in the tubes, and the burning out of the tubes, indicate that he was running a cracking process?

A. No, it does not. As I have already pointed out, in any case where tubes, or stills are subjected to intense direct fire, and particularly when we are handling crude oil, and still more especially crude oil containing water, which Pielsticker mentions as one of the products that he handles, there are very likely to be deposits of dirt, salt and coke, cemented together, and making places where local overheating can take place, even though there is no cracking whatever, taking place in the main stream of oil, and no suggestion of a cracking process, but merely local decomposition, due to these overheated films.

The Master: The tubes you refer to are not his preheating tubes are they?

Mr. Wiles: Yes.

Q. Now, I notice you mentioned salt as occurring when you are running water bearing, or wet crude oil—where does that come from?

A. Practically all the water which occurs in crude petroleum, as it comes from the ground—and, by far the major portion of crude petroleum does contain water,—is salt water, and therefore, contains various concentrations of dissolved salts, which tend to deposit, as soon as the water is evaporated—which, normally, take place at the point in [fol. 861] the pre-heater where you reach the boiling point of water at the prevailing pressure. Beyond that point you are very likely to have salt deposits.

Q. Now, is it, or it is not a fact that tubes of this kind can, and do frequently coke up badly, without any chemical change in the oil, at all?

A. I would not go as far as——

Q. I mean any change that you can measure.

A. Without any observable change in the character of the products which come out—of course some change must have taken place, locally. Some change must have taken place.

Q. What happens in this first coil of Pielsticker?

A. The principal thing that happens in the first coil of Pielsticker, is the heating up of the oil to a considerable extent, and the evaporization of the lighter ends of the crude oil, or salt solution, or whatever may be charged.

Q. How does that compare with ordinary pipe still practice?

A. That is exactly what happens in ordinary pipe still practice.

Q. I do not know whether you have stated whether the pipe still is used extensively and is generally familiar to those skilled in the art.

A. Yes, it is becoming increasingly popular for operations such as handling crude oil.

Q. After the pre-heat coil what is Pielsticker's next element, and what are the uses of it?

A. In most forms of apparatus Pielsticker's next element is the still, which is shown in figure 6, on page 1668, and also shown with certain baffle plates and as connected up to the coil in figure 11

The Master: That is what he calls the retort.

The Witness: Yes. It is also shown in Figure 4, the inlet I in Figure 4 coming in from the pre-heater, and in this still there is a separation of the vapors which have been formed in the pre-heater and the tar which drops to the bottom of the still. The tar is then further heated by the fire in this setting and more vapors are distilled off.

Q. What relative temperates are indicated for the coil outlet and the still?

A. Referring practically to Figure 11, it will be noted that the two furnace settings are substantially identical in size and construction. I would therefore draw the same inference which plaintiff's expert drew, that they con- [fol. 862] templated, roughly, equal amounts of heat application or firing on both, in the absence of any definite statement on this point. That would mean, since both the parts are almost completely surrounded by furnace gases, save for their ends, that heat losses would be negligible and, further, since with any ordinary crude oil vaporization would take place both in the coil and in the still, taking up the heat of vaporization, I should expect substantially equal rise in temperature in each part of the apparatus. Of course, a person might choose to operate with a greater portion of the temperature rise in the coil or greater portion in the still, but one would expect them to be roughly of the same order of magnitude. In other words, if one maintained in the still "I," a temperature of 700 to 750°, suitable for light

cracking, one would expect that the oil would be heated to around 400° in the coil and heated the rest of the way in the still.

I see, by the way, that I have made an error in referring to Figure 11 as showing the pre-heater coil and the still. The right hand part of the Figure 11, marked D′, is a similar construction, but is a secondary cracking coil which Pielsticker mentions the use of. What I should compare is the drawing in Figure 2 of the pre-heater coil, and the drawing in Figure 4 of the still. However, the two coils appear to be very similar in size and construction.

The Master: Was it designed to have any cracking take place in the pre-heater?

The Witness: I believe not.

The Master: It was merely to raise the material to a temperature before it went to the cracking coils?

The Witness: For most of the operation he does not contemplate cracking. I mean for his first described, apparently preferred, method of operation, he does not talk about cracking or pressure operation. It is a plain continuous distillation process and what he apparently wants to do is to put in part, somewhere in the order of magnitude of half of the heat in the coil and the rest in the still.

The Master: When you say in the coil, you mean the pre-heater?

The Witness: The pre-heater, yes.

The Master: Because there are two coils here.

The Witness: Yes. The final coil in Figure 11, has a different purpose, and he mentioned cracking in that, as I shall shortly point out.

Q. You have mentioned temperature up, say, 400 for the [fol. 863] coil and 750 for the drum. What effect would that temperature have?

The Master: You are using confusing terms. At least, they confuse me, and will probably be confusing in the record. If you say the pre-heater and the drum, then we will understand.

Mr. Wiles: Yes.

The Master: Unless you expressly refer to the letters in the patent.

Q. What effect would such temperatures have on a rapidly flowing body of oil?

A. There would be no cracking, with the exception of this almost imperceptible local decomposition in the coil, even if it went to temperatures of 500 or higher, in fact up to around 625.

The Master: In the pre-heater?

The Witness: In the pre-heater, but in the still at temperatures such as 700 to 750 there would be a slight amount of cracking, depending, of course, on how long the oil was held in there and the exact temperature.

Q. Would that cracking be suitable for the making of kerosene, increasing the kerosene yield, as was desired in Pielsticker's day?

A. Yes, and along the lines of his suggestion, that you might want to produce cracking in small amounts. Such cracking might make a very marked increase in yield of kerosene produced.

Q. For the reasons you have stated in connection with your discussion of the composition of kerosene early in your testimony?

A. Yes, a relatively small amount of cracking will make quite a marked increase in the yield of the kerosene.

Q. In Pielsticker's normal operation what is the pressure in the different parts of his apparatus?

A. In his normal operation there is no mention of pressure. In fact, he speaks later specifically of adding pressure; and, further, from the fact that he provides a steam ejector to get his tar out of the still, the steam ejector being marked P and Q in Figure 6, and also P and Q on Figure 4—which would, of course, not be necessary for operation under pressure when the tar would come out very readily. It seems to be clearly indicated that he contemplated substantially atmospheric pressure in the still, and hence, in [fol. 864] his subsequent partial condensers U, U', and so forth, shown in Figure 4.

The Master: Now, wait a minute. What do you say about page 1,671, lines 105 and 106?

The Witness: That is the part that I am coming to; that is where he adds the pressure. He first describes one opera-

tion and then he describes three or four different alternative operations that you can carry out. I am simply describing the first, or what we call the normal operation, now, and will take up the individual changes later.

Q. First the normal fractionation operations in atmospheric distillation?

A. Yes. Except, of course, he must supply some pressure at the beginning of the pre-heater to force oil through at the desired rate.

Q. And if that were not so he could not possibly need the steam ejector to get his tar out?

A. Yes.

Q. Has this general and preferred operation of Pielsticker any relation to any of the processes involved here?

A. Not so far as I can see.

Q. One of Pielsticker's suggested modifications is a change in the baffle plates in the retort or still, particularly recommended when the oil contains water. Has that modification any bearing on this case?

A. That modification described in lines 118 and following seems to have no bearing on this case.

Q. Before considering the next modification, I want you to state briefly what was the general practice of the industry regarding the use of domes on stills?

A. The use of domes on stills in the oil industry seems to be almost universal, although undoubtedly there are certain special exceptions. In the early stages of operating any batch still, in order to handle as much oil as possible, the oil is, after it is heated up, generally fairly near the top of the still, and in order to increase the height between the point that the vapors are taken off and the level of the boiling oil, and thus cut down entrainment or foaming over or priming over, it is quite general to take off the vapor from the top or the side, generally the side, of a raised part of the still. In other words, it is simply an expedient for enabling you to run the still nearly full and yet minimize the danger of priming or foaming over by carrying too much entrained particles over with the oil. In addition to [fol. 865] this function, which is all it is really intended for, there is always a certain minor amount of condensation although usually these domes are lagged, but not always, and you may frequently get one or two per cent of

condensation of very heavy ends taking place in that dome and running back into the still.

Q. Now, I wish you would consider next that modification of Pielsticker's process involving the use of a somewhat enlarged dome, R, upon his still; what is its size and function?

The Master: What figure of the patent is that?

A. It appears from the description— —

Mr. Wiles: Figure 5.

The Master: Yes, I see it here.

A. (continued). It appears from the description on page 1671, line 68, that this dome R shown in figure 5 of the drawing is a substitute for the smaller dome R shown on figure 4. This smaller dome on figure 4, it will be noted, is practically entirely surrounded, except for the top cover, with the brick work, and therefore the amount of condensation in this dome would be considerably smaller than in the ordinary type of dome which is not enclosed in the furnace setting.

I might also mention the fact that the dome R—enlarged dome R shown in figure 5 is erroneously referred to as figure 3 in this patent. It actually is figure 3 in the British patent and they have simply made a mistake in the reference.

Judging by the connections, since this dome R is to replace the other dome R, if we compare the size of the base connection on the still, and also the size of the outlet pipe S with the connection available on the still and the size of the pipe in figure 4, it appears that the scale of this detailed drawing in figure 5 is approximately three times the scale of the drawing on figure 4; in order to arrive at an idea of the probable relative size of the two structures.

The Master: Those are very uncertain inferences as applied to a mere patent office drawing.

The Witness: It is the best we can do. That is about all you can say. He makes no disclosure in that respect, except he does say that Dome R, may be enlarged at its base forming a sphere and may be provided at its lower end inside with a cup-shaped projection R and so forth.

In other words it is a moderate enlargement of the dome R. From the written description and from the detailed drawing you would judge that the detailed drawing was about three times the size of the other.

[fol. 866] Q. The flanged bottom in the two is just the same in construction, is it not?

A. Well, it does not give the same details exactly. The detailed drawing is more conventional in nature than the drawing of the still.

Q. But if the enlarged dome on figure 5 of the American patent were to take the place of the dome R in figure 4, both at the flanged ends which fit the still and at the vapor line connection at the top it would have to be reduced in the proportion that you have mentioned?

A. Yes.

Q. That is all the information you have about how big it was intended to be?

A. Yes.

The Master: Anyone skilled in the art would understand that he was to make the dome large enough to effect the purpose for which the dome was designed.

Mr. Wiles: Yes.

The Witness: Well, there is no definite enough statement on that point to guide us at all.

The Master: His knowledge on the art would guide him on that, would it not?

Mr. Wiles: Precisely, and I think that is quite right, excepting we do not want it made big enough to accomplish a whole lot of new functions. Now, we are going to show what it is intended for in the size that it ought to be, but not—it might do a lot of things, if it were something different and I do not want those read into any disclosure which is unsupported by the specification and inconsistent with the apparent showing of the drawing.

The Master: I never did take much stock in the arguments from a drawing to scale to a patent office drawing. They are only intended as diagrams to instruct those who practiced the art what to do. They are supposed to have knowledge of it.

Mr. Wiles Surely, providing there will be a useful function—that this will perform in the size as illustrated. However, that is a fair indication, and I think there is.

The Master: If somebody so altered the structure as to perform some new function, then he might make the invention.

Mr. Wiles: Exactly. That is what I am getting to.

The Witness: If this is, in fact, the scale of the drawing, and it seems to be the only reasonable assumption we can [fol. 867] make, then the lower spherical part of the dome would be practically entirely enclosed in the same setting, and the upper part would extend beyond the setting and presumably be exposed to the air. Such a device, particularly with the baffle plates shown, would be a very fine device to knock out entrained drops. The use of baffle plates to suddenly change the direction of the vapor is very effective in knocking out entrainment and there would be a certain small amount of condensation of heavy ends similar to that which the prior art frequently disclosed, and which would, generally, take place in the domes of both the atmospheric and in some of the pressure stills of the prior art.

If this scale be correct the cooling surface of the dome is approximately one per cent of the total heating surface in the still and the preheater, whereas, as I pointed out, Humphreys' disclosure in his patent and the practical operation, has more cooling surface than heating surface.

In other words, relative to the heating surface in the stills, Humphreys' device has one hundred times the cooling action, about one hundred times the area exposed of Pielsticker's device.

In other words as I see what Pielsticker did, he quite effectively knocked out entrained drops and condenced possibly one or two per cent of very heavy ends, but did not even approach or attempt a solution of Humphreys' problem of condensing eight to ninety per cent of the vapors.

The Master: If Pielsticker had conceived of that function he would probably have said something about it in his patent.

The Witness: He would have.

The Master: And he would probably show the device of sufficient size to accomplish the function, would he not?

The Witness: That seems to be a very reasonable assumption. Of course, if you had Humphreys' definite disclosure of condencing out all of the insufficiently cracked material, even though he did not show sufficient size, one skilled in the art might translate it, but that is quite different from the situation here.

Q. So that this thing, if made of the small size would have a perfectly useful purpose?

A. Decidedly.

Q. Pielsticker evidently contemplated some pressure in some of his operations. What information does the patent give us about the amount of pressure that he was going to use?

[fol. 868] A. The statement which he makes with reference to the use of pressure is that beginning in line 106 which reads as follows:

"When it is intended to produce lighter gravity oils a valve B may be placed between the outlet of the retort L and the condenser U, which may be kept open only so far as to create pressure of the gasses inside the retort, or such pressure may be created by means of a steam blast introduced in the gooseneck S and blowing in the opposite direction to the flow of vapors which distill over from the retort, or the gooseneck S may be contracted at some point, *c*, to a much smaller diameter."

This casual and indefinite reference to "a certain pressure" and a similar indefinite reference to presure in line 5, page 1672, are the only mentions of pressure.

In other words, there is no statement whatever as to the magnitude contemplated and we must therefore look elsewhere if we are to get any conception of how much pressure Pielsticker really did propose to use.

Now, of course, conceivably he can get any pressure he desires with his valve so that gives no particular information as to what pressure he desired.

The alternative scheme in which he says that such pressure may be created by means of a steam blast and so forth is, of course, a thing on which we do not have much actual operating data, but it is very hard to conceive of anyone suggesting a mere blowing upstream of a steam blast producing a pressure other than a very small pressure of a few pounds. So that would seem to be the order of magnitude that Pielsticker was probably contemplating.

The constriction, *c*, shown in figure 5, is also, at least as shown, a very minor constriction such as could produce only a very slight increase in pressure since, presumably, the unconstricted pipe is large enough to permit operation without pressure.

The Master: Does it not amount to about the same as this: that when it is intended to produce still lighter grav-

ity oils, the valve B may be placed between the outlet of the retort L and the condenser U, which may be kept open only so far as to create a slight pressure of the gasses inside the retort, the pressure being such as to result in the production of lighter gravity oil?

[fol. 869] The Witness: Yes, but how much. One pound pressure would make the gravity perceptibly lighter; five pounds would make it quite a little lighter; five hundred pounds would of course make it very much lighter. If he gave some definite criterion as to how much lighter the oils had to be, we might have some hope.

The Master: Would you understand, as one skilled in the art, that you should, by the means pointed out, select your pressure with reference to the gravity of the oil which you intend to produce?

The Witness: Do you mean the pressure to be part of Pielsticker's disclosure, or would a sensible person——

The Master: Yes, I mean a part of his disclosure, his disclosure being not to one unlearned in the art, such as myself, but to one learned in the art such as you are; that you would understand that you were to, if it was your object as he states, to produce lighter gravity oils, and furnishing you the means to apply the pressure, that you were to apply the pressure from your understanding sufficient to obtain the oils of the gravity that you wanted to obtain?

The Witness: Well, of course, if you ask me as one really skilled in the art, I would never dare to put more than two or three pounds on such a still with crude oil going into it, because I know I would blow up the still if I tried to do any considerable cracking.

The Master: That may be the answer.

The Witness (continued): The use of pressure of any considerable amount, with any considerable amount of cracking would bring up the very same problem that we have been up against right along. We now know it is not possible.

On the other hand, a few pounds pressure would make a little greater yield of kerosene which was what Pielsticker was interested in.

It could be obtained by all of the means which he disclosed for obtaining that pressure, that is, the backed-up steam blast.

I would assume that Pielsticker was taking about pressure of the magnitude of a few pounds and that he was not contemplating anything original like going to high pressure distillation at all.

The Master: You infer that because you think his structure is not adapted to stand high pressure.

The Witness: Not so much the structure but his stock as well as his structure.

[fol. 870] He fed crude oil into the still. He fed a crude oil so heavy that it takes a steam ejector to get the tar out of the still.

The Master: Yes. Of course, it was much lighter, was it not, when it came out of this preheater than it was when it went in?

The Witness: No, it would be heavier. It is specifically heavier. Of course, it would be heated up which would tend to thin it out, but you also distill off all the lighter ends; so that as to the gravity of the hydrocarbons it contains it would be heavier than what went into the preheater. It would be a reduced crude when it came out.

Those orifices, just as shown in the drawing, would be efficient in producing moderate pressures.

Q. Now, I note that this constriction is illustrated only in connection with the baffle plate dome device of figure 5. in the intended operation which you have just considered. What light does this illustration throw on the nature both of the pressure and of the re-fluxing?

A. The inference involved in showing these two together is that, first, if the pipe were not constricted we would have atmospheric pressure, because the constriction is the means of producing pressure. It therefore is true that if any considerable amount of the vapors were condensed and returned to the still by this device, that would more than counteract the action of the constriction. In other words, if we condensed fifty per cent of the vapors, or anything even approaching that, there would be so much less vapors go through the constriction that the constriction would not be effective in producing pressure. From the fact that these two are shown together and independent of anything else, one could reasonably say that this disclosure either involved only a very small reflux or only a very

small pressure. It could not possibly have a large amount of both combined.

Q. Do the drawings throw any other light on the amount of pressure contemplated?

A. Again if there are any definite statements, of course, I quite agree with the Master that the showing of the drawings is not pertinent, but in the absence of such definite statement it is only details which throw any light on what [fol. 871] he did have in mind, or would give any indication to one skilled in the art of what he had in mind; and this is especially true in an apparatus patent where he does go into a considerable amount of detail regarding the exact construction of the different parts of the apparatus. I therefore think it is significant to note by referring to Figure 6, which gives the detail of the still that it has flat end plates and comparatively quite thin walls. In other words, I think it would be reasonable again to assume from this that Pielsticker was not contemplating any considerable pressure, because if you had any considerable pressure the end plates must always be bulged.

The Master: If you were going to build a model of this Pielsticker to exhibit to a court, how Pielsticker worked in comparison with this Lewis & Clark—is that it, you are comparing this with Lewis & Clark?

Mr. Wiles: Well, this has been cited against a number of things. It has been cited against Humphreys——

The Master: Well, it applies to any of them, then.

Mr. Wiles: Yes.

The Master: You would be obliged to construct it so as to make it work, if possible, not constructed so it would not work?

The Witness: To make it work for the purpose disclosed, yes.

The Master: Yes, and of course looking back over these early devices in any art, it is always difficult to keep later knowledge from modifying them. But with all the knowledge that there was at the date of this patent, would one skilled in the art make that so as to stand the high pressures, so as to close these valves and raise the pressure necessary to produce kerosene?

The Witness: Well, it is not necessary to raise the pressure to any considerable extent to produce kerosene in con-

siderable quantities. In fact, you can produce it at atmospheric pressure, as is done regularly by the art.

The Master: Could he with the then knowledge of the art have made this device just as described to raise the pressures sufficiently to produce gasoline?

The Witness: In what quantity?

The Master: The quantity is not in question with me. I am asking you.

The Witness: Well, almost any device, even at atmospheric pressure will make a certain amount of gasoline.

The Master: I am trying to find out how much this tells to those skilled in the art?.

[fol. 872] The Witness: Well, possibly the easiest way to do it is to summarize. Pielsticker first disclosed an atmospheric pressure method for the distillation of crude oil. He then adds to that disclosure the statement that you can create "a certain pressure." Now, Pielsticker was a man thoroughly familiar with the practical art, it would appear from many of the statements in here; Pielsticker is a man who has met this problem of burnt out tubes and cleaning out tubes, and there are a great many indications of that kind. If he had been talking about a revolutionary thing in the art, like distilling crude oil at twenty or thirty or forty pounds pressure, as some others like Young did, he would have come out and said so. But in my opinion he was simply saying, I believe that a small amount of pressure, has a useful function and can safely be applied in this device and will increase the production of lighter oils. He went to a still different modification, which we still have to come to, in case he wanted to make considerable amounts of quite light oils like gasoline. There he passed his vapors through that last cracking coil, and there he did show a method by which you could, for example, make considerable quantities of gasoline without the hazard of a direct fired still under pressure, doing considerable cracking on a crude oil. I fully believe that Pielsticker and all those skilled in the art knew then that it would not be feasible to do any considerable amount of cracking of a crude oil in a direct fired pressure still, where the pressure was more than a very few pounds.

Q. I want to put to you another question right along the line that the Master put to you, on the duty of one to con-

struct this so it would work. Just as it is shown, Mr. Wilson, just as you have tried to describe it here, it would perform, would it not, an extremely useful function at that day?

A. Yes, much better than any early disclosure which I can recall of a pipe still type of apparatus for distilling crude oil, or very possibly for distilling sea water or other products as well. A very useful function.

Q. And the pressure on it—a small pressure,—might make it make even more kerosene than it normally would?

A. Yes, sir.

Q. And if you put anything approaching modern cracking pressures on, you will blow the still up?

A. If you simultaneously try to do any appreciable amount of cracking—and there would be no purpose in putting on a pressure, unless you wanted to do more than a trace of cracking.

[fol. 873] The Master: Something was said here in this testimony, of some time when boiler pressures were limited to a hundred pounds to the square inch—was that back in 1892, or 1890?

The Witness: I can't be sure of that, your Honor. I know that the modern high pressure steam—that is, pressures of the order of magnitude of four to five hundred pounds—is quite a recent development, but I cannot say what the practical limits were at different times in the past.

The Master: At any rate, do you know, or don't you know whether pressures above a few pounds to the square inch, were availed of in the gasoline art, back in 1892?

The Witness: There was no gasoline art in 1892, that I know of—except the tube cracking processes—no considerable pressures were used. The practical art in the oil industry, was all at atmospheric pressure—that is, up to 1912 or thereabouts.

Q. Now, is there any other indication that Pielsticker contemplated only light cracking in this still that you have been considering so far?

A. Well, as I more or less indicated, I think the fact that he is using crude oil and trying to avoid burning out his coils, indicates that he knew enough about it, and was anxious enough to avoid troubles incident to coke settling

on the bottom of the directly fired still, that he would not want to carry on an extensive cracking of the tar. Further, as I said, he does say, in line 131:

"If the manufacture of permanent gas, or of very light oil is intended, the retort, L, is directly connected with the second coil, D', similar to the first one, D, steam, which may have previously been super-heated, being, in preference, simultaneously admitted, with the oil vapors, into the second coil, D', by means of a steam injector, *d*, provided for the purpose. The second coil may be kept under pressure by means of a valve, *e*, or otherwise, and may be connected with an expansion chamber, or gas holder and condenser of well known construction."

In other words, Pielsticker shows that if he wanted to make gas, or very light oil—in other words, to do heavy, or extensive cracking, he first had to get rid of the tar in the still, so he took merely the vaporized materials, which would be much more suitable for further cracking, and passed those through a high temperature tube cracking process.

By inference I think that means that he could not produce that cracking, practically, in his other form of apparatus.

[fol. 874] Q. Now, I want you to turn to Pielsticker's next modification, which involves the use of the column still, W, of Fig. 10—please describe the use of that modification.

I might say that these were all cited against different things, and the only way to handle them is to take them all in succession, while we are on this item.

A. I might first say that the next modification does not happen to be in the same order as disclosed in the patent. They are being taken up in a slightly different order, I believe.

It is rather difficult to be sure, at least at the first reading, just how this column still W, is inserted in the apparatus, because both the English and the American patents have some plain error in their disclosure—I mean, one thing or the other must be wrong. I read, beginning with line 97:

"Or, instead of this arrangement, the products of distillation, as they pass over from the retort, L, may be fractionally separated in a number of oils of graduated spe-

cific gravities, by connecting the heating coil with the column still, W, which may be similar to that used in the distillation of benzole, the body of which column still I also divide by baffle plates, X, and a draw-off cock, V, for the tar, or residue, with steam injector, Z."

Now, as you have probably noted there are certain statements there which are somewhat inconsistent, because he first says that the products of distillation, as they pass over from the retort, L, may be fractionally separated, and then, two lines further, he says this is done by heating with the column still, W.

In other words, either the column still, W, is connected with the outlet of the heating coil, or with the outlet of the still, and one clause says one thing and one says the other.

If we look further, though, there are three additional indications, which point in one direction, so that I presume we will have to go with the majority.

These three additional indications are, first, the fact that he speaks of the draw-off, B, for the tar. Now, if he were merely taking the vapors, or the products of distillation, from the still, in accordance with the first line, there would be no tar. There would be some condensate or light liquid, but it would not be tar. Whereas, if he was taking the products as they come from the coil, he would have both tar and vapors, and he would use the bottom of the tower, for [fol. 875] his separating chamber, and the upper part for fractionation, just as in ordinary pipe still practice.

A second thing which also points in that direction, is his use of the steam ejector, Z, which, as I pointed out, would only be necessary because he had a good deal of fairly heavy tar, and you will note that the steam ejector, Z, is of practically identical construction, in Fig. 10, with that shown in Fig. 4. The presumption would then seem to be, with three things pointing in one direction, and one pointing in the other, that the clause "from the retort, L," should be stricken out. This rather strong presumption receives further verification when we look at the British patent, which shows essentially the same figures, but has two letters which are absent on the American drawings.

If you refer to Fig. 10, page 1668, you will note, at the top, that there is a sort of a dome, and a vapor line and a valve.

Now, if this column still is connected to take the products of distillation from the retort, then it is connected beyond the small dome, R, and the valve *b*, as shown in Fig. 4. But if, as the other place in the American patent indicates, it is connected to the heating coil, then the dome and vapor line and valve at the top of Fig. 10, would essentially take the place of the similar structures in Fig. 4.

Now, we cannot tell from the American drawings, whether they do, or do not.

But when we refer to the British patent, plaintiff's "Exhibit No. 133," it will be noted that the top of the column still—the dome at the top of the column still—is called P, and the valve is called R, which exactly corresponds with the system of lettering used on the dome P, and the valve R, at the outlet of the still, which we had already found reason to assume was replaced by the tower.

The Master: Do you find anywhere in the combinations of his claims, any claim of the tower, W—any indication that he claims the use, anywhere in the system, or that he conceives of a system in which that tower, W, is used?

The Witness: I have not studied the claims.

Mr. Wiles: Of course we conceive that it is intended to be so, but——

The Master: But the question is, what did he conceive? Did he conceive an apparatus, or combination, in which he made use of that tower?

Mr. Wiles: We think he made use of it, but that when [fol. 876] he used it he used it in place of the still, instead of in addition to the still.

The Master: I understand that, but the question is whether he claimed it, at all.

The Witness: I find no mention of the tower, in reading over the claims. That refers to the American patent. I will read the British patent, in which the claims are somewhat different.

The Master: I am referring to the United States Patent, in my question.

The Witness: I see nothing in the American patent. I find no reference to the column, W, in the claims of the British patent, either.

Q. Suppose, Mr. Wilson, that the column W is a substitute for the still, as you have contended, rather than an addition to it: Would the apparatus have a useful function?

A. Yes, a very useful function, being quite similar, as I indicated, to the present development of the pipe still art, where a coil discharges directly into an unheated separating chamber and the vapors rise through some form of fractionating device.

Q. And, considered in this way, Pielsticker's device would appear to be about twenty or thirty years ahead of the development of the oil industry in that type of device, would it not?

A. Something on that order.

The Master: Would it, then, the tower W, correspond to what has been heretofore referred as the dephlegmator, or would it perform the functions in the same manner as the bubble tower of Lewis & Cooke?

The Witness: Comparing it with Lewis & Cooke, the differences are so many that it is hard to enumerate them all. In the first place, of course, there is no showing whatever of pressure in connection with this modification. In the second place, there is no disclosure of cracking in connection with this. There is, as I have indicated, no indication of anything like a cracking process being carried out in that coil. In the third place the tower returns nothing to the body of the oil of the still, because there is no body of oil from which it comes, and there is no still to return it to. Furthermore, even if it were connected to the still, there is nothing to show any return of the large amount of stripped condensate to the still or the handling of cracked vapors. In addition to that, there is no indication that it is a bubble [fol. 877] tower. It speaks of it as the type of tower commonly used in the benzol industry, a column still. That may mean any one of a variety of different types of towers which were used there, and he speaks of the tower as being filled with baffle plates and the showing in the drawing where the bottom part is broken away, shows only baffle plates, so there is no reason to think he had a bubble tower or would get the useful function of Lewis & Cooke, obtained by bubbling the vapors through successive pools of liquid.

Q. Plaintiff's experts cited Pielsticker against the bubble tower of Lewis & Cooke, which you have just discussed in answer to the Master's question. He also cited Pielsticker against Adam's principle of indirect heating, of a bulk supply of oil, against Ellis's principle of maintaining a body of

oil at a cracking temperature without heating it enough to raise its temperature. I will have to come to those more in detail, but while we are on Pielsticker, I want to ask you whether Pielsticker shows these features?

A. No, he is very far from any such showing, because he clearly intends very considerable firing and heat introduction into his still to raise his temperature above that of the coil outlet.

Q. Even plaintiff's expert on cross examination said that he believed that he would put equal amounts of heat into both.

A. Yes. Probably put roughly equal amounts of heat in, based on the showing of the setting, which, of course would very substantially raise the temperature in the still above that of the coil outlet; whereas Ellis and Adams are limited to a body of oil held at a temperature lower than that of the stream which comes into it—lower than it or equal to it.

Q. Plaintiff's expert also cited Pielsticker against the Clark tube still cracking process. I wish you would compare these.

A. It really bears no resemblance to the Clark tube still cracking process, because of course Clark's essential feature was the use of an unheated drum and the recirculation of oil from and to the bulk supply, where the cracking is taking place, neither of which Pielsticker showed.

Q. You have mentioned another group of improvements, in the Burton process which you spoke of as of a mechanical nature, of minor importance but convenient. I wish you would state what these are.

A. These are the Burton safety valve—of patent 1168689. The Clark highline, the Cash stuffing box described in No. 1,230,065, and certain features in a high pressure type of cracking still described by Humphreys in U. S. Patent No. 1,582,929, the latter having issued since this case began.

The Master: Any stuffing box and any safety valve might be imposed in such a system in exchange for those specials shown by these patents, might they not?

The Witness: Well, these special improvements are things which are to replace the older forms shown in the original design of the apparatus, is the way I would consider it.

[fol. 878] Q. They are improved devices for this particular process?

A. They are relatively minor mechanical improvements which have their particular utility—greater safety or convenience or cheapness of operation.

The Master: One of your licensees might or might not use your stuffing box or your safety valve, or might find one that he liked better?

The Witness: Certainly.

Q. What is the fact about the use of the safety valve?

A. So far as I know, the Burton safety valve is used in all the installations of the Burton process, and I believe also in the ordinary installations of the Holmes-Manley process, and I think in a good many tube and tank processes. I am not sure on that point. I cannot say of my own knowledge regarding those two. I know the Burton and the Holmes-Manley use it very extensively.

The Master: Are they used because they are part of your general license, or are they used on their merits, because it is a superior device?

A. Well, I do not know of any compulsion on the part of anyone to use anything merely because it is covered by the patents. For instance the Cash stuffing box does not happen to be used, the Burton safety valve does happen to be used, and the licensee might make certain change as he wishes. We make certain recommendations as to the preferred form of apparatus. Those are generally followed, but there is no compulsion, so far as I know.

Q. You would not use it yourself if you knew of something better?

A. No.

Q. Now I want you to state the importance and function of this safety valve and compare it with the prior art cited against it, which was Atwood and Gaster.

A. I would not class it as an extremely important feature in cracking, but it is a kink which tends to promote greater safety and in many cases decreases the hazard involved in the pressure still fire, and of course a pressure still fire is one of the most feared things in this art. The essential feature involved in the safety valve is that instead of allowing the vapors which are at a temperature where in general they will flash,—that is ignite on mere exposure to the air on account of being at such a high temperature—instead of [fol. 879] allowing them to come in direct contact with the

air itself, to pass them through a relatively short length of pipe to give them the slight degree of cooling which is necessary to carry them below the auto-ignition temperature, before they are allowed to come in contact with the air.

Before we compare ths with the prior art it is necessary to realize what a very important function, not this particular safety valve, but any safety valve must perform on a pressure still, as distinct from an atmospheric pressure still. About the most that will ordinarily happen in an atmospheric pressure still is to have to take care of an amount of vapors, somewhat similar to those which are ordinarily coming off through the ordinary vapor line, in case that becomes clogged or the condenser becomes obstructed, or the rate of firing is a little higher than it should be. But in a pressure still we have the situation of a very large volume of oil, possibly 13,000 gallons in a Burton still, or still more in a Holmes-Manley still, which is super-heated not only to its own boiling point but a couple of hundred degrees above it, so that practically all the stock in the still will flash into vapor if the vapors are released, and make a tremendous amount of fire. Furthermore, the safety valve is set ten or fifteen pounds above the normal operating pressure, so that it practically never blows except in case of a fire, as, for example, due to a split bottom or split tube, which greatly increases the rate of heating and raises the pressure from, say, 90 or 95 pounds, the normal operating pressure to 110 pounds, where the safety valve may be set.

The Master: Mr. Myers had something to say about your having patented a piece of six-inch pipe. Is this the one?

The Witness: This is the one.

Mr. Wiles: This is the one, in a particular place, for a particular purpose.

The Witness: Again, the apparatus is admittedly old.

The Master: Why confine the piping to six inches; why not take it off sixty feet?

Mr. Wiles: Because it would spoil it, it would not do the thing that is intended to be done at all, and Mr. Wilson will show you why. That is what the art did and that is what we cannot do.

The Witness: I am still talking about any type of safety valve on a pressure still whether it is the Burton type or any other type.

[fol. 880] The safety valve must then dispose of the entire contents of the still, get it out of the still before the pressure in -he still reaches a point whcih would disrupt the whole still and make a catastrophe; and that, in general, means, from our experience in pressure still fires, that the still contents should be emptied in approximately fifteen minutes. That is the basis on which we design our safety valves. They must handle something like fifty thousand cubic feet of vapor per minute measuring it at atmospheric pressure and still temperature. Or, to put it in another way, where the ordinary condensing system of the still will condense something like fifty per cent of the still contents in say fifteen hours, here we are handling one hundred per cent of the still contents in fifteen minutes, so it would mean that we would have to have a condenser something like one hundred times as large as the ordinary condenser if we were going to do as the prior art frequently did, attempt to condense these vapors to prevent their escape.

Furthermore, we would have to have a tremendous condenser of this type for almost every still, because when there is a fire frequently three or four adjacent stills are affected, and if we had a single condenser for a number of stills it would have to be still bigger in order to handle simultaneously all the stills that might be affected.

You cannot have a long vapor pipe running, a long distance to a condenser, because the friction in such a pipe of any reasonable size would again prevent you getting this tremendous amount of vapors out in the short time that is available to do it. If you tried to do a partial job of condensing, you get into worse trouble than if you do not try to condense at all, because the tremendous rush of uncondensed material sweeps the condensed drops of liquid along.

If, for example, you made a pipe as you suggested made, more than 60 feet, say 150 feet long, you would probably cool the vapors enough to condense quite a considerable proportion but those drops would be swept along and instead of being still vapors which would ascend in the air and get away from the fire, you would spray a mass of oil right down into the fire, which would be the worst thing that could happen.

The essence of the Burton invention is by putting a short length of pipe, beyond the safety valve, a matter of five or

ten feet as a rule, which is just enough to give the slight [fol. 881] cooling necessary to prevent the auto ignition of the vapors as they come out of the still, but not to condense anything, so that the vapors will rise and get out of the way and postpone ignition just as long as can possibly be done.

That, of course, brings out the necessity of doing slight cooling and no condensing, which in itself shows that Burton was doing exactly the opposite of what the prior art tried to do on atmospheric pressure stills, and which is perfectly proper for atmospheric pressure stills. That is, the latter tried to condense and save the product which came off when a safety valve blew. Atwood, for example, provides an auxiliary condenser in which the safety valve blows and tries to condense a considerable proportion of those vapors; and Gaster, Exhibit 127, conducts them to a reservoir with no outlet, presumably where they will all, or substantially all, condense.

Neither of those come anywhere near meeting or solving Burton's problem, which he solved in quite a different way.

Q. As I recall it, Mr. Wilson, plaintiff's expert took the position in considering this patent that when a safety valve on a pressure still blew it just let out a few pounds of pressure and stopped, the relief being relatively small and brief. That is not the fact, is it?

A. Not the way we ever operate our pressure stills. I do not know what the ordinary general practice may be..

The Master: In this emergency when you get a fire underneath your still and your pressures are running up quickly and you must have relief to save an explosion and you release into the air just above the fire, do not the vapors take fire?

The Witness: They sometimes do eventually, but very frequently we can confine the fire to the fire box. That is, we try to cut off the supply of air and open the damper wide and try to confine the fire to the fire box. Now, that is a very hot fire in there. It is ten times as hot as the ordinary fire, but in many cases it does not get up around the outside of the still, but if this torch from the safety valve is ignited, as sometimes happens in fires when the fire escapes from the still, the radiation of heat from that torch is such that I have seen it set fire to coal bunkers of cars of coal by pure radiation maybe fifty feet away. This radiation from the tre-

mendous torch of fire very greatly endangers the surrounding stills. It may eventually catch, and frequently does, but every minute that you can postpone it gives you that much more time to get the situation under control.

In many cases most of the visible fire in a case of that kind is at the top of the stack. There is not enough air getting into the setting to completely burn the oil which escapes and it passes through the flues and up the stack and burns at the top of the stack.

The Master: Then it is more likely to ignite your vapors coming through?

The Witness: No, the stack is always kept very well removed from the stills and is much higher than the still.

Q. And these pipes are considered—these cooling pipes beyond the safety valves are considered substantial protection in case of fire?

A. Yes.

To the best of my knowledge the Clark high line is used by all of the licensees of the Indiana Company. That is covered, by two patents, one on an apparatus and the other on a process.

[fol. 882] The Clark high line is a very convenient method of increasing the regularity of operation of the stills and decreasing the amount of labor and attention required to operate them. Formerly the operation of an individual still required quite close attention in order to prevent wide fluctuations in pressure and variability in distillate, but by connecting the gas space of all the receiving drums on a large battery of say sixty stills, it becomes possibly to regulate the pressure on all those sixty stills at a single point, with only one man responsible for that task.

Furthermore, this affords at once a convenient supply of gas, (since the stills are operated on an overlapping cycle) for backing into the stills at the start of the run, to prevent distillation and for making up any temporary deficiency in the amount of gas which would be produced in a given still.

The latter feature is probably not one which is very frequently needed but every so often, due to carelessness in firing, and particularly if there is a variable wind which makes it hard to fire with regularity and which affects the condensation of the runback system, a still may run slowly, as it is termed, for a considerable time. That is, the amount of distillate coming over would be distinctly less than the

normal amount and less than the amount of gasoline which was being cracked, the extra amount of distillate in this still being condensed and returned to the still; however the lightest distillate and the gas that is made during that period keeps coming on over into the house all the time. Now, in an effort to correct that the fireman is given instructions to fire that still harder and in his zeal to get it back on to a normal schedule he sometimes fires it too hard and heats it up a little above the proper temperature, and all of that considerable amount of gasoline which has been made and collected in the still comes over in a comparatively short time; whereas, the only amount of gas that is available to saturate that distillate, if it were operating as a single still, would be the amount of gas that is being currently made, the rest having left the system; and as I say, in many cases that would not be enough to saturate this abnormally large volume of distillate. What happens then is what is known as a pressure drop, a reduction of pressure, and the minute the pressure reduces even a small amount a lot of the intermediate boiling material in the kerosene range has its boiling point lowered and it begins to distill over. That increases the difficulty, sets up a vicious cycle, and as the pressure tends to drop quite rapidly a large amount of material comes over into the receiving house. Of course, the condenser is unable to handle that. I have seen the paint on the receiving drum blistered in such a situation, and it is only by having automatically available gas pressure to restore the pressure and stop that action that we can get a stable and really satisfactory operation.

Of course, when the stills are connected to the high line, the amount of gas which any still requires to prevent the start of such an action is comparatively small, but it is nevertheless a very important though small quantity of material.

Q. That small quantity comes in automatically without anybody even knowing it is coming in, when it is needed?

A. Yes. Of course, there are occasionally times when the gas on the whole battery has not gas enough to saturate the distillate, but that happens comparatively seldom, although a few cases are known where we lose pressure on a whole line and that creates quite a serious problem.

[fol. 883] Q. You have completed the description of the structure and operation and advantages of the Clark high line structure, and plaintiff's expert cited Dewar & Redwood in that connection—what have you to say upon the applicability of this reference?

A. Both the similarity between and the difference between those two structures seem to be quite obvious.

The use of the high line to stop distillation after the dehydration period, or near the beginning of the operation, is very similar. The greater convenience and simplicity and cheapness of the Clark scheme of supplying gas thereto from other stills, instead of pumping in air or other gas, is of course, obvious, though the monetary cost is not greatly different.

The greatest advantage of the Clark high line, over the scheme of Dewar & Redwood is the advantage of single point control, due to having a whole battery hitched up, with only one point for pressure regulation.

The Master: I cannot recall—is there what might be termed a high line in Dewar & Redwood?

The Witness: There is just a line into which gas is pumped.

The Master: There is a line?

The Witness: There is a line into which gas is pumped.

Q. But in Dewar & Redwood it is for a single still, is it not?

A. Yes, sir.

Q. And the still has its individual pump, and its individual gas outlet.

A. That is the showing of Dewar & Redwood.

Q. Whereas, in Burton, there are several stills and pumps for each of them, and you have a single outlet for perhaps sixty or more stills?

A. Yes, sir.

Q. Now, would you agree with the position of plaintiff's expert, taken on page 576 of the Record, that the Clark arrangement enables you to run the plant with less gas?

A. Yes, it enables you to run the plant without compressing any outside gas. The plaintiff's expert seems to grant the novelty of Clark's scheme as a superior arrangement for its purpose.

Q. You mentioned, in connection with these so-called mechanical improvements, which you said were of minor importance, the Cash stuffing box. Tell us about that, briefly.

A. The Cash stuffing box, which is described in patent No 1230065——

Mr. Wiles: Just a moment. I will offer this patent as defendants' "Exhibit No. 408."

The said patent, so offered, was admitted in evidence, was marked "Exhibit No. 408," and the same is herewith returned.

Q. Now, you may proceed with your answer.

A. If we want to produce any internal agitation, or carbon scraping action inside of a pressure still, we have a very serious problem in getting a rotating journal or other device, through the still wall and yet not have trouble from leaks, on account of the high pressures and high temperatures involved, against which it is hard to get packing material to stand up, and the fact that if oil does leak out, there is a considerable fire hazard involved—indeed, it may flash into flame as soon as it reaches the air.

To overcome this difficulty, Dr. Cash proposed an ingenious arrangement of a double stuffing box, with a chamber between the two, into which relatively cold oil is forced, at a pressure equal to or slightly higher than the pressure [fol. 884] of the oil in the still. In that way any leakage in the inner stuffing box, which is the hard one to keep tight, is inwardly, where it does no particular harm, and the outer stuffing box can be kept tight, quite readily, because the temperature of the oil which is trying to leak through, is very much lower, and even if a little should leak through it would be cold oil and not hot oil.

This device is not actually used on any of our Burton stills, because we do not use carbon scraping or agitating mechanisms at the present time. If has been tried out experimentally, and has worked very successfully, and might very likely be found convenient in other structures, where we did wish to have carbon scraping or agitating mechanisms.

A. This includes the essential developments and improvements over the prior art, which have been made in connection with the practical operation of the Burton process.

Q. Now, in general, in going over this, I have asked you specifically the extent to which most of these inventions were used, but I think I neglected to ask you the extent to which the Burton re-run invention was used in the Burton process, both by the Indiana Company and its licensees.

The Master: Was, or is?

Mr. Wiles: Is.

A. The Burton re-run invention——

Q. And you may include its use, so far as you know it, through the period of the use of that process.

A. The Burton re-run invention is very extensively—in fact, used to almost the maximum possible extent, in all of our refineries. This makes a larger increase in the total amount of available charging stock, than might be thought, at first glance. At the end of each individual cracking operation we only have 40 to 45 per cent, of pressure tar, and part of that must be left as a bottom in re-distilling of it. But when that material is charged again, to the pressure stills, it, in turn, leaves a 40 to 45 per cent. bottom, which must be re-run, so that the total accumulation in what we call cycling to completion, and that we use to figure out ultimate yields, is such that, as a rule, including some re-running of the bottoms from pressure distillate, we generally charge to our stills, more than twice as much oil, [fol. 885] altogether, as of fresh stock, itself, and we can say, roughly, that the re-run process nearly doubles the amount of available stock, as compared to what it would be if we only had fresh gas oil, or other virgin stocks to charge to the stills.

The Master: I don't get your arithmetic on that.

The Witness: Shall I put it in the record? I can do it on the blackboard—probably more——

The Master: Do it whichever way you please.

The Witness: This is just the situation. Suppose we have a hundred parts of pressure charging stock and get from it 45 parts of tar, which will give us about 40 parts of cycle stock. Then, from that we will get, on re-cracking again, 45 per cent. of tar. This gives us 40 parts of cycle stock here (indicating). When we re-crack that we will again get 45 per cent. of tar—it will amount ultimately, to 40 per cent. of that, which gives us another 16 per cent.

added on, but we re-run that again, and we have a 45 per cent. bottom of that, which gives us another small increment, and then if we add in, also, the heavy ends of the pressure distillate, which we re-run—that is, we take it off as distillate, and make our gasoline out of it, and put a little of the heavy ends of the pressure distillate back in, and it makes the total amount of material charged to the still, twice, or more than twice as great as the amount of fresh gas oil which is charged to the still.

That is, the fresh gas oil is a hundred parts, first cycle stock is forty parts, second cycle stock sixteen parts, third cycle stock will be over six parts, and fourth cycle—each time you re-crack the cycle stock you have left a bottom which is cleaned up and run again.

The Master: Your statement it that you have twice as much as you put in, and that is what is confusing. I do not understand how one can have twice as much at the end of the operation as he put in.

The Witness: On account of the fact that, say, we only make thirty per cent of gasoline per pass, per individual run in the still, and that we put back most of the remainder of the material, and then take thirty per cent of gasoline off that and put back most of the remainder of the material, and then take thirty per cent of gasoline off that and put back most of the remainder, the total amount of stock there, as you put it into the stills, is more than twice as great as the amount of fresh stock that goes to our stills. In other words, on the average the stock pasess through our stills more than twice.

[fol. 886] The Master: Oh, yes, sure. I see now. You are charging the same stock to your stills.

The Witness: Re-charging the same stock.

The Master: And you are re-charging the same stock to your stills over and over again.

The Witness: Yes. Another way to put it is that after the first few runs we would have about fifty per cent of fresh gas oil and the other is material from previous runs.

Q. All I was trying to bring out is that almost half of our operations are based on this re-run patent and the other half on fresh oil.

A. So far as I know this has been practically universal practice ever since Dr. Burton first found that it was a

possible and profitable operation; because we have had no other satisfactory way to utilize the great bulk of this material. With regard to our licensees, the greater portion of them re-cycle practically as much as we do, some, particularly I believe those located on the eastern seaboard, where they can buy very heavy reduced Mexican crude for fuel oil, use this tar which is quite fluid to mix with the heavy oil, and reduce the viscosity so they can get a better market price for it, and in that case they do not re-cycle to anything like the extent that we do.

The Master: How many cycles are there in the Burton practice?

The Witness: Well, the cycles are in a sense never ending, but it amounts to, as I say——

The Master: In the sense of distinct operations—distinct recharging operations.

The Witness: Well, you see, we do not segregate cycle stock and put it back by itself, we simply put it into our charging stock. If we were starting a new plant, the first time through we would have a hundred parts of fresh charging stock. Then we would make from that maybe fifty parts or fifty-five parts of total cycle stock, including naphtha bottoms, which would go in with the 100 parts of fresh gas oil; in other words, the second time through maybe our charge would be about one-third cycle stock and two-thirds fresh stock. Then from that we get some additional cycle stock which we put back in, and the next time the proportion of fresh stock in our total charge will be still less, and finally we settle down to a condition where our stock consists of less than half of fresh stock. If you [fol. 887] tried to follow any particular molecule, you might find that some of those molecules had gone through pressure still operations, hundreds of times but on the average we will say each molecule goes through a little more than twice before it is either cracked to make gasoline or to make heavy tar or some other product which is eliminated from the system.

Q. You mentioned the so-called high-pressure still. That is set up in the answers of the Indiana company to the plaintiff's interrogatories as one of the processes that it uses, and the answers to interrogatories apply to it. I

wish you would tell us about this high-pressure process that is there referred to, Mr. Wilson.

A. This high pressure still described in U. S. Patent No. 1,582,929 to R. E. Humphreys, is a development which was designed specifically to take care of a new clean stock produced by the operations of the Burton stills themselves, namely, the heavy ends of the pressure distillate which are not suitable for gasoline, and which are formed in not very large amount per pass, but which total a considerable quantity. If we put that into our regular charging stock, as we did for a long time, it of course increases the volatility or lowers the boiling point of the stock, and therefore makes it harder to get higher still temperature, at 95 pounds pressure, and yet good distillate—the old question of the proper correlation of temperature and pressure in a bulk distillation process.

Q. Correlation to the stock.

A. Yes, correlation to the stock. These naphtha bottoms cannot be cracked by themselves at ninety-five pounds at a rate which even approaches a commercial rate, but it was found, that by taking advantage of the improvements in the boiler-making art and taking advantage of a number of improvements in construction, that they could be cracked at quite a rapid rate at pressures above three hundred pounds, and the socalled high pressure stills were therefore designed for the specific purpose of getting a rapid cracking rate, on rather low boiling stocks.

There are a number of novel features in the construction. In the main it is based on the general principles of the Clark tube still with thermo-siphoning action to cause circulation through the tubes, but instead of having a single drum there are several drums of small diameter which will stand the pressure, together with special forms of connection between the two. There are seven of these [fol. 888] stills operating at Whiting at the present time, to take care of a considerable portion at least of our naphtha bottom production.

I also have a diagrammatic drawing of the still, together with the other parts of the equipment, showing the essential arrangements.

Mr. Wiles: The patent just mentioned is offered as Defendants' Exhibit No. 409.

Mr. Rauber: What is the number and date of that patent?

Mr. Wiles: May 4, 1926. And I offer the drawing as Defendants' Exhibit 410.

Mr. Rauber: What is the number of the patent?

The Master: No. 1,582,929. The application was filed January 7, 1921.

(The said patent so offered, was admitted and read in evidence, was marked "Defendant's Exhibit No. 409," and the same is herewith returned).

(The said drawing so offered, was admitted and read in evidence, was marked "Defendants' Exhibit No. 410," and the same is herewith returned).

Mr. Royce: Mr. Wiles, before you go on, may I ask was this diagram Exhibit 410 prepared by and under the direction of the witness?

Mr. Wiles: That is my understanding.

The Witness: Yes.

Mr. Royce: He had not testified to it and I wanted to understand it.

The Witness: Yes, that applies to all these diagrammatic drawings of the stills. As I say, I do not certify them as to minor details, but they give a simple picture of what happens. Possibly a very brief description of certain points might not be out of order.

The operation is very much like that of the Clark tube stills, as described yesterday, I believe, except for the fact that we have a large number of small drums interconnected in order to stand the relatively high pressure better and yet have a large body of oil. The reflux stock and the feed which is introduced into the base of the tower it will be noted pass into the lower header in accordance with the invention of Burton and Clark, rather than going into the stills and mixing with the stock in the stills.

Q. That Burton and Clark has not yet been discussed, I understand, in detail?

A. No.

[fol. 889] Q. Does this conclude the main developments in the practical embodiment of the Burton process up to date?

A. With the exception of that Burton and Clark patent, yes.

Q. Which is used only in the high pressure still today, as I recall it?

A. I believe so. At least as far as Whiting operations are concerned.

Mr. Rauber: May I have the name of that patent?

The Witness: The high pressure still?

Mr. Rauber: Yes.

The Witness: The Humphreys patent.

Q. I think it is clear, or at least I want it to be if it has not been, to what extent the so-called Burton process varies in its practical embodiments in general.

A. We have, as I believe is brought out in the answers to the interrogatories, a rather large variety of combinations of different forms of stills, with different forms of reflux equipment. Thus we have both tube stills and shell stills, the tube stills being the Clark form, with radiators runback, operating in many of our refineries, and we also have a considerable number of tube stills with Lewis and Cook bubble towers overhead. In addition to that, of course, we have the high pressure stills which involve certain other features.

Q. Those variants you generally refer to as the Burton process in one of its forms.

A. Those are generally considered to be variants in the Burton process, carried out on different types of equipment with the aid of different later improvements.

Q. What other practical developments have there been in the general art of cracking by bulk pressure distillation?

A. The other main improvements in the art of cracking by bulk pressure distillation in a general way are, first, those seeking to make the operation more nearly continuous. That is a problem which our company has been much interested in for many years and all companies realize that it would be very desirable to conduct cracking more as a continuous operation instead of having to shut down so frequently for cleaning, and thereafter reheating the setting and incurring strain in the metal. All those factors are involved in the operation of a batch process with a cycle only 36 to 48 hours long.

Q. Mr. Wilson, right at this point I do not think we have [fol. 890] brought out just what this cycle is. We did bring out the time consumed, in dehydrating and digestion, and generally the time taken by distillation; but then I want you to show in addition the time that this stopping and cleaning out takes so as to bring the whole cycle together.

A. On the shell stills which I first described the dehydration step takes from three to five hours, the digestion step from five to eight hours, the distillation step from twelve to fifteen hours; and then two to four hours cooling down and five or six hours, something of that order as a rule, for cleaning out the still, putting on the manhole plates and charging. We call the complete cycle the time from starting firing to starting firing. The cycles are almost always made in even multiples of at least four hours, and preferably even multiples of eight hours, so as to fit in with the time the men come on shift, so that a new shift of men come on when firing starts, and so forth. The cycle on the shell stills is generally a 36-hour cycle. The cycle on the tube stills,—where the running time is increased to around twenty or twenty-two hours, on account of the fact that we feed into the stills during the run is ordinarily forty-eight hours in length.

The Master: How many shifts of men on that forty-eight hour cycle?

The Witness: We have eight hour shifts.

Q. I would like to have the Master,—if you can give it to him as I have acquired it by seeing it,—I would like to have him get some picture of just what this cleaning means, just how the men go in and how clean they really get.

The Master: How clean the men get or how clean the apparatus gets?

Mr. Wiles: How clean the stills get.

A. The cleaning operation is one in which we insist on very great care and very rigid inspection. In fact the assistant superintendent of the department is supposed to give the final inspection before the stills are passed as ready to begin operations. But of course he puts most of the responsibility on the stillman and foreman in charge, and if he finds that he has to reject any stills, he generally points out to them that they must be more strict. The cleaning is

such as to bring all the fired surfaces in the still, whether the bottoms of the shell stills or the tubes in the tube stills, to practically a mirror-like finish. They do not allow any thin layer of adhering coke to remain, because even a very thin layer of coke will have some insulating action and will [fol. 891] tend to gather oil, which becomes overheated, and causes this excessive local deposition of coke. The cleaning between runs is a very important part of the operation. Of course, in the Clark still the cleaning of the drums does not have to be conducted so carefully because that is not exposed to direct heat, and frequently the Clark stills are run on a cycle, which calls for the cleaning of tubes every run, but only cleaning the drums every other run. In other words, there is quite a bit of coke-holding capacity, and you can collect coke there without its doing any harm.

Q. What was the first development in this direction of getting greater continuity, a more nearly continuous operation, than you had before?

A. The first step in that direction was that taken by Burton and Clark in the socalled "boot" patent, which is in evidence as Patent No. 1,448,254. In this operation, instead of returning the reflux and the feed which has come in along with the reflux stock in accordance with the Clark patent, directly into the still, and allowing it to mix with the stock in the still before it goes through the tubes, which tends somewhat to lower the still temperature, the feed and reflux is conducted down to the entrance of the tubes by an interior funnel, as shown in that patent, which is placed under the point where the runback returns to the still. This means that the stock going through the tubes is both cleaner and somewhat cooler than would be the case if we take the stock from the bulk supply as a whole. So it tends to lessen coke deposition in the tubes for both those reasons, and at the same time tends to keep the still temperature, a little higher, because we do not have the cooling action of this stock which, while it is heated fairly close to still temperature, of course cannot be heated exactly to still temperature. This is a step in the direction of prolonging the runs by minimizing coke deposition on the heated surfaces.

Q. Now, Plaintiff's expert cited the Adams patent, No. 1320354, against the Burton & Clark boot patent—leaving aside the legal question as to whether Adams is properly

part of the prior art, in view of his later publication date, I want you to please compare the two processes.

The Master: What is the Burton patent that you are talking about?

Mr. Wiles: That is the Burton & Clark patent No. 1448254.

A. The essential feature of the Burton & Clark patent is, [fol. 892] as I indicated, the returning of the reflux stock, together with the feed, to the heating tubes, rather than to the bulk supply. Adams shows two different forms of operation, in Fig. 1 and 2. In Fig. 2 you will note the series of chambers of increasing size, at the upper left hand corner of the drawing, No. 126, 128 and 132—at least, those numbers are on the drawings.

The three chambers increasing in size, from right to left, through which the vapors pass, undoubtedly producing very substantial condensation of insufficiently cracked material.

But in this form of operation these insufficiently cracked products do not return directly to the flowing stream, but return to the supply tank W, from which they may be recracked in a different manner.

The other method of operation is shown in Fig. 1, in which there is a return of some material which left as distillate, such as may be condensed in the first knock-back chamber, 115, being the number inside of the chamber. Here, however,——

The Master: 113 is the number of the chamber and 115 the number of the baffle plate.

The Witness: That is probably correct. This may optionally return the stock directly to the heating, zone, but I will call your attention to the fact that here the amount of condensation is far less than in the other patent, which provides for fairly complete condensation of the insufficiently——

Q. You mean the other figure?

A. Yes, the other figure—which had a series of three chambers of gradually increasing size, to accomplish that purpose. I should say that what would be accomplished by the single chamber, arranged as shown, would be mainly the knocking back of entrained drops, together with a little condensation, more or less incidental, of the heavy ends of the

distillate—very much like what is accomplished by several of the other prior art domes.

The Master: Assuming that W is the original tank of charging stock—is that what you assume in Fig. 2?

The Witness: That is not the original charging stock, but it is pumped from that tank by the pump R, up into the tank B, and re-run from that point. There is no return direct to the restricted heating zone in Fig. 2.

The Master: That disposition of the uncracked products, is not the disposition at all, that Burton and Clark made of it, is it?

[fol. 893] The Witness: No, that is the point—that is not the same, at all.

The Master: Would it be a reference, at all, on that subject?

The Witness: No. The only thing which is a reference is this relatively small knock-back, and return, which is shown in Fig. 1, where you do return direct, and that, as I say, is not, in any sense, Humphreys' type of reflux. The material is simply disclosed as uncracked material and entrained drops, and, of course, the arrangement of the baffle is quite effective in knocking out entrained drops which might be carried over by the vapors which rise through the vapor dome, and are carried over through the vapor line.

So that the differentiation between this and Burton & Clark, is the same differentiation as between Pielsticker's run-back, for example, and Humphreys' run-back—one is a small amount of entrained bottoms, and unconverted heavy ends, and the other is the return of substantially all of the insufficiently cracked material—that is, Humphreys' is the return of substantially all of the insufficiently cracked material.

The amount of condensation which Adams got, in Fig. 2, would, I should say, be an amount comparable to that of Humphreys, but that does not go back directly to the heating coil.

Q. Now, plaintiff's expert also cited Pielsticker against Burton & Clark—what have you to say on that point?

A. Not only is Pielsticker's reflux, as I have shown, inadequate, in comparison with Humphreys', but it does not return to the flowing stream.

Q. It goes back either directly into the still, or into the supply tank?

A. Yes, sir.

Q. Just like this figure——

A. Like Fig. 2 of Adams—it returns to the supply tank, and Pielsticker optionally returns to the still, or to the supply tank.

Q. But not to the tubes direct?

A. But not to the tubes direct. Of course, returning any large volume of reflux, such as Humphreys contemplated, to the supply tank, would mean a gross waste of heat, which would enormously increase the difficulty and cost of operating the process. With a small amount of reflux, such as in Pielsticker, the logical place to put it is in the supply tank, and the amount of heat involved is comparatively small, anyway.

[fol. 894] The Master: What other losses, if any, are there in returning these entrained drops, as you say—I don't mean that—I mean returning to the supply tank the uncracked material, as in Fig. 2 of Adams.

The Witness: The loss of heat is the principal thing, but if, of course, the reflux is 80 to 90 per cent. of the distillate, as in the case of Humphreys, the loss of such an amount of heat is a terrific loss. In fact, you could not return Humphreys' reflux direct to the supply tank, because, when the pressure was reduced it would flash into vapors. You would have to put it through an actual cooler, first, before it could go to the supply tank.

Q. All the heat you took out in the cooler, would be waste, that would have to be supplied again in the tubes, when you fed it in?

A. Yes, and that, of course, is not only a waste of heat, but it means harder firing of the tubes, which increases coke difficulties again.

Q. Now, what is the principle drawback in this Burton & Clark method, and how, and by whom was this drawback overcome, if it all?

A. The principal drawback in the Burton & Clark method, which kept it from making any very great increase in the length of runs, was that while it did insure the feed and reflux going direct through the tubes, it was still mixed with

a considerable quantity of dirty stock, which had to be recirculated in order to keep the body of oil in the still, at a rapid cracking temperature. This difficulty was overcome by Holmes and his associates, who made this, and a very considerable number of other improvements, which were finally embodied in the Holmes-Manley process developed by the Texas Company, and in a somewhat modified form, in a continuous pressure still process, developed at Whiting.

Q. Now, I believe the Master requested, during the examination of plaintiff's expert, that at some time the defendants should describe the ordinary meaning of the terms continuous, intermittent, batch, and semi-continuous operation.

Now, I wish you would please state the significance of those terms, at least as you use them in your testimony.

A. I used the term continuous process, primarily in contra-distinction to a batch process.

The essential features of a continuous process are that over any considerable period of time the amount of ma- [fol. 895] terial put into the system shall almost exactly equal the amount of material which leaves the system in different forms.

And, further, that the composition, temperature and pressure which is maintained at any given part of the system, during the operation, is substantially the same—after, of course, the parts have been brought to their operating temperature—after, in other words, the starting up period.

Of course none of these factors can be held exactly constant. There are always fluctuations in pressures and temperatures, and some changes and variations in the composition of the stock, and even in the liquid levels in the system. But as a general rule, the amount of liquid in the system, and its composition, and the amount of vapor in the system, and its composition, are roughly the same throughout the running period, and the feed in it is made large enough to balance what is taken out of the system.

The Master: That is, they are ideally constant?

The Witness: Yes, they are ideally constant. In a continuous cracking process, on account of the slow accumulation of coke, even in the best designed process, occasional shut-downs are necessary for cleaning, and it is, therefore,

perfectly true that the continuous process is also an intermittent process. In other words, intermittent is not the opposite of continuous. The term intermittent applies in the cracking art, at least, to both batch and continuous operations, and the term continuous is primarily a term which draws a contradistinction to a batch operation.

In a batch operation, in general, we start with a given lot of material and operate on it under changing conditions of temperature, pressure and composition, and generally with a decreasing volume in the system, until the end of the operation is caused by coke deposition, or something else.

So, again, the batch processes are necessarily intermittent, and, generally, are interrupted at more frequent intervals than the continuous processes.

In addition to this, we have what we call semi-continuous processes, in which we do feed into the system a certain amount of material, trying to keep the volume of oil somewhere nearly constant. But we do not draw tar, or remove, in any way, the worst coke forming constituents, and while we can run for a longer time, it still is essentially different from a continuous process, and generally runs a much shorter time than a truly continuous process, and we, in the cracking industry, frequently refer to this as a semi-continuous process, to distinguish it from either the batch or continuous processes.

[fol. 896] The Master: I want to get the distinction between the batch process and the continuous process—that distinction being that in the batch process you have given a quantity of material——

The Witness: To start with.

The Master: To start with, which is operated to finality of operation?

The Witness: Under changing conditions.

The Master: Under changing conditions.

The Witness: And generally decreasing amounts.

The Master: But in the continuous process you do not start with a given quantity. You start with the capacity of material, of your apparatus, and then continuously feed into it fresh material, of the same nature?

The Witness: Yes, sir.

The Master: Of course, there being a period, as in all apparatus, when you have to stop for the cleanout.

The Witness: Yes, sir.

The Master: Now, in the semi-continuous process, what happens?

Mr. Wiles: Can we finish the continuous process, while we are on it? There is one other point that the witness brought out.

The Master: Yes.

Mr. Wiles: In the continuous operation, you not only feed fresh material, but you also withdraw tar, so as to keep the composition constant throughout the operation, after the startup—after the period when you strike a fair balance.

The Witness: In other words, if you allow——

The Master: But that is only an incident. It doesn't deal with continuity of operation.

The Witness: Yes, it does, in this sense, your Honor, because, if you do not withdraw tar, then the composition of the material in the still is getting dirtier and dirtier and dirtier, by the accumulation of these heavy ends. Whereas, if you do withdraw tar from the system, then in time, the composition of the material in the still, becomes practically constant—you are getting rid of the dirty stock as fast as it is formed.

The Master: So that you are maintaining a certain equilibrium in your material?

The Witness: Yes, sir, and the difference between that and the semi-continuous process, is that you do feed in, in the semi-continuous, but you fail to draw out, so that your stock in the still is always varying in composition, and generally you make some variation in the temperature to correspond to that change in composition.

Q. Now, just to pin this down to specific things—you would call the original Burton process, as practiced in batch stills, without feeding, a batch process?

A. Yes, sir.

Q. Now, when you take the Clark tube still, where you do not pump out, but do pump in, you would call that semi-continuous?

A. Yes, sir.

Q. Now, this Holmes-Manley process, that you are going to take up next—how long does that run at a time?

A. Fifteen to thirty days, as a rule.

Q. And after you have run it perhaps a day, and hit your balance, you pump in and withdraw so that the stock is the same—the contents are the same for the whole fifteen days?

A. Substantially so. Of course there is an accumulation of coke, which is not removed, but, with possibly slight changes, the stock in a given still is substantially the same over that period.

Q. After you get really started you are running what you would call truly continuous?

A. Yes, sir.

Q. Even if you would have to shut down sometime to clean up?

A. Yes, sir.

The Master: Of course, we don't understand by continuous, that it shall be eternal.

Mr. Wiles: No. We hope to arrive at that point, but we have not yet.

[fol. 897] The Witness (Referring to model): This represents a rather detailed model to 1/24th scale, of the Holmes-Manley continuous cracking process, which is I think almost universally agreed to be the highest embodiment of the art of bulk pressure distillation.

To get our bearings a little, in comparison with the art as developed by Burton and Clark, you will recall that even in the semi-continuous Clark process the total amount of oil handled is about twenty thousand gallons in a cycle of two days, or about ten thousand gallons a day, with the making of thirty to thirty-five per cent of gasoline; whereas the Homes-Manley process puts through approximately seventy thousand gallons every day, or seven times as much as the Clark process, and makes around forty per cent of gasoline, and runs for a period of fifteen to thirty days, and frequently longer, before having to be shut down because of coking.

It is obvious that that has required a number of important and far-reaching improvements in the art of bulk pressure distillation, particularly with regard to preventing the accumulation of coke when doing such heavy cracking over such a long period of time without shutting down.

To first get a sort of birds-eye view of what the apparatus is, before we trace out the flow lines in detail, I will point out that this is the receiving and control house where the instruments are placed which indicate the temperatures at different points and which measure the amounts of feed or distillate, and permits of taking samples of feed or dis-

tillate, the operation being watched and to a very considerable extent controlled from that house.

The oil after passing through the meter in that house goes to the pre-heater, which is this bank of tubes under a roofed-over shelted near the stack, and then to the stills, which are four vertical stills, ordinarily four feet in diameter by forty feet long, or five feet in diameter by forty feet long, with walls in general around three inches thick, equipped with internal carbon-scraping mechanism and fired very lightly on the sides only, below the liquid level and above the pocket where coke scrapings tend to collect. The stills are in that other brick structure, the house above, the interior of which we will later inspect.

Q. At this point, Mr. Wilson, I wish you would bring out the sense in which you use the term "stills" or "still" for this construction.

A. The four individual drums in this device are in common refinery practice referred to as stills. They really amount to a single sectional still with a common vapor line and liquid overflow from one still to the next, but the vapors going off in parallel, giving a single product from the four stills.

The Master: Is it there that the digestion takes place?

The Witness: It is there that the digestion takes place, and by far the greater portion of the cracking takes place.

The vapors from those stills pass into the bottom of the large bubble tower which is surrounded by what we call a dephlegmator, although that is not in exact accordance with [fol. 898] the use of the term in other parts of this case. It is a partial condenser which serves to condense heavier ends of the rising vapor by means of the cold feed oil which is circulated inside a coil, though it does not come into direct contact with the vapors. The condensed vapors flow back to the bubble tower in the ordinary manner and the lighter uncondensed gasoline product passes over to the final condenser box.

Q. That is the black box here, that you are talking about?

A. That is the black box with the coils in it.

There are some other intermediate products besides the main stream. There the tar is drawn from the bottom of one or more of the four stills and passes through the heat exchanges to the first box.

Also the reflux condensate at the bottom of the bubble tower, which is at a temperature of around six hundred degrees, is handled by a vedy ingenious type of pump known as a surge pump, which permits of pumping very hot liquids. While this reflux is under pressure it has to be given the additional pressure necessary to force it through the coils in the preheater, and then back to the still.

Having gotten a birdseye view, I think we will take up the detailed course of travel of the oil through the system. This discussion so far, until I mention a change, is taking place while I stand on the side of the stack in front of the small black box, which is the tar cooler. The feed oil comes from the high pressure pump, not shown in this model, which supplies oil under, say, five hundred pounds pressure, or more in some cases, to a large number of these units, from a common manifold, passes into the larger of the two black lines in the receiving house, past the Venturi meter which records exactly the amount of feed flowing in at any minute, and of course this can be controlled by the valve in the feed line; passes down to the ground, up on the outside, and I trace along a line which goes to a valve at the lower left corner of the condenser box, where there are two possible passages for the oil. It can if desired be sent up through the dephlegmator or vapor heat exchanger on top of the tower, or it can go straight ahead past this point (indicating).

The ordinary operation is to pass such a proportion of the feed to the dephlegmator as is necessary to condense all except the desired final product. In other words, instead of allowing a certain amount of air cooling surface to condense all except the desired product, as Humphreys first [fol. 899] disclosed, this condensation is produced by circulating some of the cold feed through the dephlegmator, thus giving a more positive and accurate control, and also recovering the heat in a useful form, instead of rejecting it to the air.

Of course, Clark by introducing feed directly to the vapor line picked up part of that heat also, but the amount of feed in Clark's case was much smaller than in this case. That part of the feed, which is not required to pass through the dephlegmator, bypasses around the dephlegmator, being controlled by the small valve with the chain. That determines how much is allowed to pass around the dephlegmator.

Then we come to another division point, where the feed can either pass through these tar heat exchangers, picking up waste heat from the tar, or can bypass those tar heat exchangers and go direct to the preheater.

In the continuous stills at Whiting, I may say, we do not have these tar heat exchangers, but use the contained heat in the tar to distill off the cycle stock for reintroduction in the stills, but in this model and most of the regular Holmes-Manley operations, the liquid passes through the tar heat exchangers.

The feed, after it has in whole or in part, or in some cases not at all, passed through the tar heat exchangers, then passes around and into the top row of the preheater which is known as the economizer section. While the feed has picked up a considerable amount of heat, it is still at a temperature of only 450 to 500 degrees, we will say, so that it is able to cool the furnace gases to a low enough point to give good fuel economy, and this economizer section, you will note, cools not only the gases which have passed through the preheater proper and up into the economizer section, but also the gases from the light firing around the stills, which are conducted through special ducts which go down into this setting above the pre-heater tubes and carry these gases through the economizer section, where they give up a considerable additional amount of heat.

After leaving the economizer section the fed passes down and joins the line coming over from the surge pump, which brings the hot reflux condensate. The two are generally run so as to be at about the same temperature when they are mixed at that point, the feed having been heated up to six hundred degrees or more in the economizer. We then put this oil into the top of the pre-heater section, and it [fol. 900] works its way down toward the bottom, being raised to the cracking temperature.

I may say that the tubes here are in general about three inches in inside diameter and 3½ inches in outside diameter, with some variations—2.9 inches in inside diameter and 3.4 inches in outside diameter—and there are a large number of tubes, as you will note, in the system. The oil is circulated very largely in parallel in the early stages, in order to cut down the frictional resistance at a point where high velocity through the tubes is not vital. But when we get into the hottest part of the system the flow lines unite

and pass in series through all the pipes, giving the very high velocity which, as Clark pointed out, is quite important to prevent the deposition of carbon in the tubes. Furthermore, by passing it at this very high velocity and by arranging the firing so that the furnace temperatures are kept quite low, Holmes and Manley are able to prevent any considerable amount of cracking in the pre-heater. That is one of the principal secrets of the very much longer runs made by this than made by any other either tube or bulk pressure distillation cracking process. The cracking and deposition of coke in the tubes is kept at an absolute minimum.

The oil is, however, heated to a rapid cracking temperature, picking up a great deal of that heat in just the last two passes, where the hottest gases and the radiant heat from the furnce strike it. So it is only in the rapid cracking range for a matter of seconds, or possibly a minute or two.

The oil, then heated to around 800 degrees Fahrenheit—possibly 810 and occasionally as high as 820 under present operating conditions—and having dropped in pressure to the extent of form 40 to a hundred pounds, depending on the amount of oil which is being pumped through, due to friction in the pipe coil, enters the stills which may be kept at a pressure anywhere from 250 to 500 pounds, or in that general neighborhood.

I will now pass around to the north side of the stills, where we can see more clearly where the hot gases pass down and into the economizer section from the still setting, and the arrangement of the stills, which is particularly made clear by opening the end of the apparatus, where we see the nearest of the four stills cut away so as to show the interior, the carbon scraping mechanism, and the second still beyond. There are four stills, and the firing in here around these stills is maintained by a series of special [fol. 901] burners which fan out a flame and given a very moderate, light amount of firing, just enough to keep a gentle glow, more to minimize radiation and heat losses from the still rather than to put in very much heat. As a rule, the amount of heat put into these stills is probably not over 15 or 20 per cent as much as is put into the pre-heating coil. But here is where the great bulk of the cracking of the oil takes place. I will ask someone to switch on the device and

we can see the operation of the carbon scraping mechanism. Of course, the speed of rotation is actually very much lower than that of this model.

You will note that the scrapers are arranged quite ingeniously to overcome the problem of having a scraper which is at once positive and yet which will not bind in case coke builds up, as it almost inevitably does with the most efficient scrapers. This is accomplished by having each individual scraping blade weighted and pivoted in such a way that a certain, constant weight is exerted on the scraping member at all times, regardless of how thick the coke may be. And as the coating of coke builds up to an eighth of an inch, they are still scraping off the top layer and keeping it uniform, although it is not all scraped off clean. The hard baked-on carbon which cannot absolutely be scraped off a fired surface by any mechanism, in time binds and gives trouble. But this mechanism does keep it uniform by scraping off the top, and agitates enough to keep the asphaltic material, which tends to collect there, suspended in the mass of liquid, oil, before it tends to form coke.

We have tried experiments, I may say, to see what would happen if we did not operate the scrapers, and instead of being able to go a matter of thirty days under ordinary operating conditions, in less than a day the stills, will begin to show red and the operation had to be stopped. The operating mechanism which turns these scrapers, is shown by standing on a chair and lifting the roof.

Those are rotating the scrapers by a chain drive, and a gear drive directly on top of the still, turns the scraper shafts which pass through stuffing boxes. The stuffing boxes are in this form of construction in a fairly cool part of the still. This is all above the fired surfaces. Also the vapor lines are above the fired surfaces.

You will note, also, looking below, the carbon-catching chambers. The scrapers operate over the entire fired surface, but, as I pointed out, the fired surface does not extend [fol. 902] above the liquid level, nor does it extend down to the lower part where the scrapings tend to accumulate.

At the end of this fifteen to thirty-day run, the space below the scrapers is generally fairly well filled up with material which has been scrapped from the walls and has fallen down in there and formed a sort of porous mass of

coke which has to be dug out. And even then the total amount of coke formed, when making forty per cent of gasoline, is approximately only one-tenth of a per cent of the oil charged to the system during that period, which is a record unequalled, even unapproached, by any other type of cracking process with which I am familiar. They start with a charging stock which is very similar to Burton's charging stock; that is, it includes gas oil, paraffin light ends, naphtha bottoms, and cycle stock from rerun pressure tar—very much the same general type of clean cycle stock.

It is possible with these improvements, in taking care of coke, that you might be able to put a little crude oil in with the stock, but your run would last but a short time, on account of the excessive coke formation by such stocks. You would sacrifice the length of the run.

You will note that there is a very large proportion of the stills extending above the liquid surface and giving space for the vapor line, the scraping mechanism, and so forth. The vapors pass out on this arrangement here into two vapor lines. Incidentally, you will note here a safety valve of the Burton type of construction shooting the vapors up and cooling them below their auto-ignition temperature. Then the vapor passes into the base of the bubble tower, which varies somewhat in construction but is a more or less conventional type of bubble tower with generally around 30 to forty plates. Here the vapors get the polymerizing action which so markedly improves the quality of the gasoline and is more effective at this higher pressure than at the Burton pressures at which Lewis & Cooke first operated. Thus the distillate which comes off is a markedly superior product, which requires very, very little refining before it can be marketed—distinctly less refining than required by the product of the Burton stills, due to the action of the bubble tower rather than due to any differences in the stills themselves.

As I said, since the bubble tower is lagged very little condensation takes place there, and instead of getting the condensation as Humphreys did or as Lewis and Cooke first showed, by direct radiation to the atmosphere, the conden- [fol. 903] sation is obtained by contact in the small device above, with a cooling coil. Of ourse, there is no vital rea-

son for separating these two units. Some new continuous stills which are being built have the cooling part directly in the top of the tower instead of in a separate chamber, and there we condense a very substantial proportion of the vapors. Generally seventy-five to eighty per cent, I believe, of the vapors which rise through the bubble tower, or at least which come into the bubble tower, are condensed and go back as reflux and the remaining vapors pass over with substantially gasoline boiling points. In fact, it is possible to make a high test gasoline with lower end-point than ordinary gasoline on this type of still tawing nothing but that out of the towers. The vapors leaving the exchanger pass down through the condensers which are split up into two in parallel, and then the condensed liquid still under substantially still pressure, passes over into the receiving house, where by a special control valve the pressure is reduced to eighty pounds.

You will note the gas is not separated in this case at the maximum pressure, because at such high pressure the gas is very soluble in the liquid, so that if you try to separate the gas there you find that you do not have much if any excess left over, and there is not much to be gained by separating. But the pressure is reduced in the receiving drum to eighty pounds, which has been found experimentally to give the best result. That is, you release a relatively large quantity of gas while it is occupying a relatively small volume, and therefore does not carry away with a large proportion of the light ends.

It is interesting to note that the pressure in that receiving drum which could be operated at any pressure between 300 or 400 pounds and zero when set for maximum savings, happens to give the optimum result at the pressure at which Burton was operating in his process of separating the gas under pressure.

The reflux liquid from the bottom of the tower, which, it must be remembered, is a type of material ordinarily considered very difficult to handle, being at very high temperatures and very high pressures—say well above 600 degrees as a rule—is handled by this form of pump known as a surge pump. Of course, the difficulty with pumping at such pressuers and temperatures is to get valves, and particularly a pump cylinder, which will stand the temperature

and not have excessive leakage and excessive wear, and which can be kept lubricated. The surge pump runs very cold. What it does is to handle a cold slug of oil over in here (indicating), which it pumps into the near pipe first. This tends to displace from the solid steel block which has been drilled out, which is merely a valve chamber, some hot oil which has previously collected. The operation is then like this: the slug of oil moves to the left due to the action of the pump, pulling in from the base of the bubble tower a charge of hot oil, and valves open up to admit it. Then the slug of oil comes back and those valves close, and other valves open which allow it to go into the feed line. Then, again, we suck in and push out, the important feature being what we might call the long piston of cool oil which serves to move the hot oil. The slug which serves as a piston, is kept from getting hot by mixing and contact with the hot oil by long pans filled with water between the valve chamber, which is hot, and the pump which operates cold. This is known as a surge pump, and is very useful for accomplishing this purpose.

The tar, you will note, can be withdrawn from the bottom of any one of the four stills. Generally most of it is drawn off from the fourth or from the third and fourth stills, and occasional slugs are taken from the first and second stills. The feeding of the liquid from one still to the next is through the liquid lines in the upper part of the chamber, and merely spills over by gravity through the lines, maintaining substantially the same level in all four stills.

I forgot to say that the feed can be introduced into any one of the first three stills. Generally about two-thirds is introduced into the first still and one-third into the second, though that is also varied at times.

[fol. 904] Q. It has been called to my attention that several times in your testimony, you have used the expression "polymerize." Will you just define that, please.

A. Polymerization is used, in this art, to refer to two processes which are similar in many respects, but different in others.

As I pointed out, first, in cracking there is always a synthesis of heavier products, as well as a breaking up into lighter products, and the synthesis of the heavier products is then referred to as polymerization. What it really means is many molecules joining to make one.

Also, in discussing what takes place in a bubble tower, on a pressure still, by which underisable gum forming consti-uents are eliminated, I have spoken of that as polymerization, because, apparently what happens is the formation of heavier products, which go back into the still, instead of distilling out with the oil.

In both cases it is the conversion of certain lighter products into heavier products.

The Master: In both senses it is a species of synthesis?

The Witness: Yes, sir.

Q. I should have asked you, when you were discussing the Burton & Clark reference, about the applicability thereto, of the Greenstreet patent, which was cited against that particular patent.

A. The Greenstreet reference seems to have no bearing, whatever, on the Burton & Clark process, because Greenstreet is simply a tube cracking type of process, and all he does is to put his heavy distillate back into the supply tank, and thence through the cracking zone again—at least, that is the only thing that I have been able to find in that patent which bears the slightest resemblance.

Q. Now, I wish you would state the features of practical importance, which were added to the art which you have already discussed, in the development of this Holmes-Manley, or continuous type of process, which you described generally with the aid of the model.

A. Probably the most important forward step which was taken by Holmes and his associates, in developing this process, was in limiting the circulation through the tubes, to clean stock—that is, either stocks of the nature of fresh gas oil, and similar stocks, which Burton disclosed originally, or the reflex stock from the fractionating tower, and not putting through either any dirty stock originally, or not circulating, or recirculating, as Clark did in his tube stills, any of the stock which had partly cracked in the stills, and had, therefore, grown dirtier. This latter procedure, [fol. 905] as I pointed out, was the fundamental difficulty with the Burton & Clark invention—because while their invention did tend to increase the proportion of clean stock going through the tubes, still there was some dirty stock recirculating at the same time.

Limiting the stock put through the preheater to nothing but clean stock, of course, is one of the principal elements in making it possible to run for a very long time, before building up coke in the preheater, to an extent sufficient to necessitate cleaning. But it creates a whole new series of problems in the still, because the circulation now being limited to clean stock, the stream which is available to supply the heat to the body of oil in which cracking, and from which distillation takes place is greatly reduced. In order to maintain this stock at a cracking temperature, Holmes and his associates found it necessary to apply a certain amount of heat to the stills. At first sight this might seem to be a step backwards from the disclosure of Clark, that it was better to carry on the cracking in a bulk supply, which was not exposed to direct heat, but the difficulties with that scheme, which many considered insuperable, were overcome by Holmes and his associates, by several expedients, some of which were disclosed by Adams in his prior patents.

One of the features which made it possible to have this firing, while considerable cracking was going on, was changing from horizontal to vertical stills. Of course, in the horizontal still the coke would always settle down to a point on the surface closest to the heat, but by changing to a vertical still and by firing on the side only, above the bottom where the liquid tar and coke settles out, and below the liquid level it is possible to do much more firing without a serious accumulation of coke on any fired surface.

But even then it is necessary to add a third element—the carbon scraping and agitating devices, which rotate inside of the stills, and tend to keep the carbon deposit thin and very uniform, although it does built up in time, to an appreciable thickness.

Even with all these precautions Holmes found it necessary to limit himself to very light firing of the stills, and particularly to making such dispositions of the flames, in that chamber, that there was no chance of impinging any direct flame, or very hot gases onto the stills, but instead a very gentle, soaking heat entirely surrounding the stills.

He also divided the still into four section stills, as I pointed out, thus increasing the amount of heating sur-

[fol. 906] face and making it easier to handle high pressures, and yet have considerable oil under treatment.

In general, the feed is put mostly into the first still, and most of the tar is withdrawn from the last still, although as I pointed out that some feed might be introduced to the second, or even to the third still, and some tar might be drawn from the third, or even the second still.

This series operation has at least two advantages over the single still—first, in that the firing can be adjusted and varied, and the temperature maintained in the still can be varied from still to still, as the composition of the stock changes. And, second, by forcing a more or less uniform flow in one direction. All the material stays in the cracking zone for roughly the same amount of time, without the danger of what we call by-passing, when working with a single still. In a single still, fresh feed, even if put into one end while the tar is being withdrawn from the other, may be carried rapidly through the still by connection currents and taken out before it has had time to undergo the amount of cracking which it should. Of course that is important only in a once through process, where the tar does not recirculate. If the tar is recirculated, as in the Clark tube still, then there is no such by-passing problem, particularly because tar is not withdrawn continuously from those stills.

Even with these devices or preventing the formation of coke on the walls, and preventing overheated walls, as I said, it has been necessary to have only light firing, and generally the oil temperature drops from twenty to 50 degrees between the outlet of the preheater and the average temperature prevailing in the stills.

In other words, part of the heat in these stills is essentially superheat in the liquid which came in.

The Master: In view of the number of times the word "tar" occurs in your testimony, don't you think you had better define the sense in which you use the term?

The Witness: I would be glad to. I will not guarantee that exactly the same sense fits all the uses.

In general tar is a somewhat generic term for the heavier, generally darker colored liquid residues that are formed through distillation or cracking. The fact that it

is an undistilled product is, in many ways, the most important distinction, but not all undistilled products are tar.

In the cracking process, after cracking has continued for any length of time, we generally refer to all the material [fol. 907] which remains in the still, or is withdrawn as liquid, therefrom, as tar.

The Master: The sense in which you use it—is it a product of anywhere near the same viscosity as pitch?

The Witness: No, it is not. In the case of the Burton tar it is a very light fluid product, black in color, and of quite low viscosity.

Of course the tars which result from distilling, or cracking crude oil, especially if there is much asphalt present, are much heavier. The resemblance is in the darkness of the color, and the fact that they are residues of distillation.

Q. There are one or two points I want to direct your attention to. What is the importance of putting most of the heat into the oil, in the coil?

A. The importance of that is to avoid the necessity of putting any considerable amount of heat in where the most of the cracking is to take place, and where coke deposition is likely to take place.

Furthermore, an additional important feature of the Holmes-Manley process, which I believe I failed to mention in this discussion, is the very rapid circulation through the coil, and the fact that the oil only reaches a cracking temperature at the very end of its passage through the coil, which makes it possible, together with carefully modulated firing conditions, to prevent too high furnace temperatures—to prevent more than a very small percentage of cracking, or only incipient cracking taking place in the tubes, and, as I pointed out, such incipient cracking can be produced with very little deposition of coke.

The Master: Will you please explain the means by which this highly velocity is produced in the system?

The Witness: That is by the scheme of circulating all the oil through a single pipe instead of putting it through a large number of pipes in parallel—as, for example, in the Clark tube still. We handle a large amount of oil, and we put it through a single pipe, using positive pressure in the form of a pump, to produce the high velocity. That is, we have forty to 100 pounds extra pressure, just to overcome

the friction in the coils; which is attendant upon that very high velocity and most of that comes near the end of the passage through the coil, because, as I pointed out, in the [fol. 908] upper part of the system, where there is no danger of cracking, the streams are in parallel so that the velocity in each section is much lower.

Q. I do not know, Mr. Wilson, whether repetition at this point would clarify things, but you pointed out to us yesterday or the day before, the time-temperature factor in cracking and I would like to have you explain just in a little more detail how this high velocity permits you to get up to temperatures as high as 870 to 840 without any substantial cracking?

A. This happens because at least the last one hundred degrees of temperature is put into the oil in about the last two passes of the preheater,—the bottom tube just above the fire,—and that means that the oil is heated to that extent in probably (I am not trying to give exact figures) something on the order of twenty to forty seconds' time. And, as I also pointed out, the amount of cracking is a function of both time and temperature. I also pointed out that in tube cracking you had to work at high temperatures to make up for the short time available.

The Master: Do I undestand that you avoid your cracking by your velocity?

The Witness: Only indirectly by your velocity. You avoid part of your coke deposition directly by your velocity, in preventing the formation of super-heated films, but the prevention of cracking is an indirect result of velocity, namely, the result of the shortness of the time that the oil is at the cracking temperature.

The Master: That is what I mean.

Q. It is only at these high cracking temperatures for a few seconds?

A. Yes.

Q. And then goes into the still, so that the amount of cracking in the coils is very small?

A. Yes.

Q. Now, in what patents are the principal features of this Holmes-Manley or continuous process first disclosed?

A. A number of the features were first disclosed in the various patents of Adams, others are in patents issued to

Manley, to Holmes, and to Beheimer, or the same men in combination. It is my understanding that other features are covered by applications still in the Patent Office.

Q. Now, I wish you would describe the diagrammatic drawing which you have prepared of this Holmes-Manley process, referring as far as necessary to the prior description.

Mr. Wiles: I will offer this drawing as Defendant's Exhibit 411.

A. This is a diagrammatic representation of the principal features of the continuous pressure distillation process carried out at Whiting, which is similar in most respects to the Holmes-Manley process. It only covers the principal features, omitting many, many details which are shown on the model.

[fol. 908½] Mr. Rauber: It is not supposed to be a working drawing of the Holmes-Manley apparatus?

The Witness: Absolutely not; simply a diagrammatic representation to clarify the essential features.

The Master: It is merely an illustrative drawing, as I understand it, to facilitate an understanding of the successive steps.

The Witness: That is correct.

Mr. Royce: It was prepared by you, Mr. Wilson?

The Witness: It was prepared by a draftsman from sketches which I drew.

Mr. Royce: You had an opportunity to check it?

The Witness: Yes.

Mr. Royce: And you believe it to be accurate?

The Witness: Substantially so.

Mr. Rauber: Does it represent the Holmes-Manley apparatus actually used?

The Witness: It represents the essential features of the continuous process at Whiting, but is substantially the same in these essential features as the Holmes-Manley process as generally carried out.

Mr. Rauber: That is used by the Standard of Indiana?

The Witness: In what I call our continuous process; I am referring to what we, at the Standard of Indiana, use.

Mr. Rauber: This is the continuous process?

The Witness: This is the continuous process, which is essentially the same in its main elements as the Holmes-Manley process.

Mr. Rauber: I do not care particularly about that; I just wanted to know whether this represented actual, commercial installation or just a model or what it represents.

Mr. Wiles: Of course it is not in proportion. What do you mean by an actual, commercial installation?

Mr. Rauber: I mean has it got everything on it that a commercial plant has?

Mr. Wiles: Oh, certainly not.

The Witness: Nowhere near.

The Master: It has no evidentiary effect whatever, Mr. Rauber. These are simply illustrations which do not prove anything. It just simply aids in understanding of what the witness is going to say.

Q. Are the features that are diagrammatically illustrated on this drawing, Exhibit 411, present in the operations of the defendants, Standard Oil Company of Indiana, at Whiting?

A. Yes.

The Master: Not a bit nearer to a working drawing than the diagrams of a patent.

Mr. Wiles: Exactly.

[fol. 909] (The said drawing so offered, was admitted in evidence, was marked "Defendants' Exhibit 411", and the same is herewith returned.)

A. (continued). Referring to this exhibit No. 411, the feed enters the system near the upper——

The Master: You denominated Exhibit 411 "Holmes-Manley" and your apparatus in use at Whiting is avowed to be a Holmes-Manley process.

The Witness: No, not exactly. It differs in a number of respects.

The Master: In other words, you are only referring now to the common features between Holmes-Manley and this apparatus at Whiting?

The Witness: That is correct.

Q. And so far as it is illustrated in this sketch the two systems are common?

A. Yes. The feed enters, as I indicated, near the upper righthand corner of the drawing at high pressure, passes through the dephlegmator or vapor heat exchanger, which

is here shown as being in the top of the tower. Of course, you will recall there is also a bypass so that some of the feed can go around that dephlegmator, if desired, and the feed then passes——

The Master: Which is done merely for the utilization of accumulated heat in that place?

The Witness: And which affords a control of the composition of the distillate which is allowed to pass that point.

The Master: Both objects?

The Witness: Both objects.

The Master: Not only to heat feed but to cool the product in the dephlegmator?

The Witness: And to condense out the reflux stock to go back for further cracking.

The Master: Yes, I see, a heat exchanger.

A. (continued). The feed at a pressure of something like 350 pounds or higher passes through the pipe shown to the economizer section of the preheater and then passes down to the heating or pre-heating coil, being joined by the stream of reflux from the bottom of the bubble tower, which I will mention later.

The combined streams then pass through the heating or preheating coil and come out at a temperature of about 800 degrees Fahrenheit and 300 pounds pressure entering mostly the bottom of the first still, but in part the bottom of the second still.

The Master: You spoke of those refluxes joining with the feed line and passing through either the pre-heating coil or the heating coil. You did not mean that the combined stream passed up through the pre-heating coil?

The Witness: Yes, the upper part is the economizer.

The Master: That is the economizer but this the pre-heating——

The Witness: The lower coil is the heating or pre-heating coil.

The Master: I understand that, but the stream, where the two streams unite at the heating coil——

The Witness: Yes.

The Master: The take-off there from the economizer, this stream of reflux cannot go above that, can it?

The Witness: No, I say they unite there and go into the heating coil.

The Master: Yes.

The Witness: It does not go back. The two come together and join at that point.

A. (continued). The material is then fed, as I indicated, at a temperature of around 800° F. mostly to the base of the first still, some generally to the base of the second still. [fol. 910] The four individual drums here are in plant practice, as I said, ordinarily called stills, but they really constitute one large sectional still with a common vapor line shown at the top and the common liquid line which maintains substantially the same level in all four of the stills or sections of the still. These stills are all enclosed in a single setting which again emphasizes their unitary feature, though it is possible to adjust the firing to get a somewhat hotter fire toward one end than toward the other end, if desired.

The stills are also equipped with scrapers of a type of construction which we saw this morning, being weighted bars arranged in staggered form, so that they cover the entire surface and are pressed by a practically uniform force against the well, regardless of the extent to which carbon may build up, but will, nevertheless, yield in case any serious obstruction gets in their way.

The flue gases from the small gas burners, which are very carefully arranged, it will be noted, pass off to the left into the preheater, and pass up through the economizer section in order to recover more of their heat.

The vapors pass out through the vapor line shown, into the base of the bubble tower, shown diagrammatically only, and rise therethrough to the top where around 75 per cent thereof are, in general, condensed. This condensate runs down the tower, being stripped substantially free from gasoline by the hot current of rising vapors and passes down as condensed reflux stock at a temperature of about seven hundred degrees. I believe I said this morning it was above six hundred; the more accurate temperature is in the neighborhood of seven hundred degrees.

The vapors, substantially gasoline in composition, leave the top of the bubble tower, pass down into the condenser,

pass the valve which controls and reduces the pressure into a gas separating drum which is operated at eighty pounds pressure.

The gas is taken off through a pressure release valve and the distillate is metered in one of several types of meters, depending on the plant.

The tar may be drawn off from any one of the four stills, but as I stated this morning, most of it is drawn from the last still with generally occasional slugs from the second and third. The tar is handled different ways in different plants. It may go to the tar heat exchangers, such as were used on the model; it may go to a tar cooler and be cooled, or it may go to what we call an auto-tar distillation plant where the most of tar flashes into vapors, something as in the Barbet patent, which you may recall; and the heavy vapors and the lighter vapors are separated, the heat in the tar being used to carry out the distillation without further direct heat.

Q. They simply shoot it into a drum at atmospheric pressure and what distills by the contained heat goes off, is that it?

A. Yes, leaving a heavy residue.

The Master: Does that conserve what vapors come off?

The Witness: That conserves the vapors that come off; the very lightest vapors being put into the gasoline frequently, but the great bulk of the vapors, being pressure tar distillate such as that of Burton is put back into the process in a subsequent operation.

Q. In other words, yo just use the contained heat of the tar as it comes out to make it distill instead of re-distilling as in the Burton re-run in a separate still?

A. Yes.

The Master: That operation is incidental to releasing the pressure on the tar?

[fol. 911] The Witness: Yes.

Mr. Wiles: While it is still hot?

The Master: While it is still hot, yes.

Q. If it did not do that—you run it through a tar cooler, do you not, to prevent that?

A. Or through a heat exchanger and then a cooler, in which case the tar may be sold to dilute fuel oil but is generally put in other stills and re-distilled separatelly to recover the cycle stock.

Q. Now, you have referred to the Burton and to the Holmes-Manley continuous process at Whiting as bulk pressure distillation processes, in accordance with the classification which you made earlier in your testimony. Please state how you classify the other two of defendant's processes here involved; that is the Cross process and the tube and tank process?

A. These other two processes——

The Master: I do not get the correlation of bulk in that connection.

The Witness: The essential reason for the use of the distinguishing term "bulk" is that vaporization, or, in a certain sense, distillation, takes place in practically every cracking process. In a tube process, as I pointed out, distillation takes place, but it lacks the large body of oil held for a long time at a cracking temperature from which the product is formed also, in a bulk pressure still the light products are removed substantially as fast as they are produced, leaving behind in the still only the material which is not sufficiently cracked.

The Master: I do not get the application of that to this apparatus.

The Witness: Here we have the——

The Master: Where is your bulk?

The Witness: In the four stills.

The Master: Oh.

Q. How much do they hold, Mr. Wilson?

A. Well, they are five feet in diameter by forty feet high and they are all of them nearly full of liquid. There is a very large bulk of oil held in those, and as I say, that is where the cracking is brought about. One of the principal reasons why the gas losses are so small in this type of bulk pressure distillation process is that the gasoline is distilled and removed from the cracking zone practically as fast as it forms, preventing further cracking, which makes more gas.

I, of course, pointed out in my early testimony other [fol. 912] characteristic features distinguishing them. For

example, I pointed out that when we are carrying out a bulk pressure distillation process we must have a very close correlation of temperature and pressure so as to distill out what we want to and hold the other things behind; whereas, in a tube-cracking process the products all pass through the tube and are in there about the same length of time.

Mr. Wiles: I think this expression was coined but we did want to draw some line so that you could not say that other processes was this kind of process, if we called it merely a pressure distillation process.

The Master: You mean you coined it for the purposes of this case in order that you might make your differentiation?

Mr. Wiles: Coined it for purposes of classification here. It is not a standard trade term. In order to draw a line between this group of processes and that group where there is always, or nearly always, some distillation, and frequently under pressure, but which worked quite differently, as the witness has stated.

Q. Now, the question was how do you classify the other two processes, the tube and tank and the Cross?

A. The other two processes are not bulk pressure distillation processes, but are essentially modified tube cracking processes which developed by way of the tube cracking art.

The essential addition which they made to the prior art of tube cracking was in inserting between the cracking tube and the separating drum of the prior art a large reaction chamber where further cracking took place with only moderate addition of heat. There is no distillation in the sense of bulk pressure distillation taking place, because both the vapors and the liquids are still held in the cracking zone, and both are released together from a single outlet by means of a valve.

Q. Now, I want you to mention again very briefly the general principles which distinguish the prior tube cracking art from the bulk pressure distillation?

A. I just mentioned some of them but I will repeat them. In bulk pressure distillation we have as the essential feature the holding of a large body of oil at a cracking temperature and pressure for a matter of hours or days. The temperature and pressure at which the oil is held are so correlated as to allow the desired product to escape from

the still substantially as fast as it is formed, and the tar [fol. 913] either remains behind in the still or is drawn separately and handled separately.

These processes are further characterized by being conducted almost invariably at fairly low temperatures and by having moderate gas losses, and as a rule, very moderate coke losses, and making a distillate which is more saturated and much more like ordinary gasoline than the ordinary tube cracking processes, particularly the prior art tube cracking processes.

The tube cracking art on the other hand is characterized by cracking a flowing stream, which may be a stream of vapor or a stream of liquid, or a stream of liquid that is partly converted into vapor as it passes through the system, but all the materials pass along together. There is no large body of oil held at a cracking temperature from which vapors distill off. Everything goes through the cracking zone together and is in there for about the same length of time.

Tube cracking processes, on account of the shorter time available in a tube as compared with a large bulk pressure still, are almost always conducted at higher temperatures. Furthermore, on account of the smaller diameter the tubes are characterically, although not invariably, run red hot toward the end, because the hazard of an explosion is far less, the thickness of the walls being greater in comparison with the diameter. Being run at a higher temperature tube cracking process tend to make more highly aromatic and less saturated types of gasoline, and, due to the fact that the gasoline is subjected to extensive re-cracking, especially if you try to do any considerable amount of cracking per pass, the gas losses are invariably higher, and, as a rule, the coke losses are higher than in bulk pressure distillations.

Plaintiff's expert mentioned twenty-one references which described or refer to tube cracking processes. However, as was the case with the bulk pressure distillation processes, the number of really separate and different processes which we need consider is less, since several references sometimes bear on essentially the same process, though it may differ in minor details. The actual separate processes are sixteen in number, or which eleven are prior to 1914 and those

bear upon the more important problems which we have to [fol. 914] consider here. Three are of a later date and therefore have a narrower application. The eleven older processes are,

First, the Benton process, described in Exhibit 150, which is Benton's U. S. Patent, and also in Redwood's book, Exhibit 107;

Second, the Schuchow and Gavrilow patent, Exhibit 109;

Third, that operation of the Pielsticker process of Exhibit 134, in which the final cracking tube is employed for cracking the vapors generated in the still. You remember that was in figure 10 or 11 at the end where he really wanted to do extensive cracking to make gas, or very light oil.

Fourth, the Breitenlohner and Vohl processes which differ in some respects but are essentially similar, described by Gruening in Exhibit 250.

Fifth, the Kornfeld process of Exhibit 123.

Sixth, the Nikiforoff, Exhibit 148, 149, 154 and 155.

Seventh, the Testelin and Renard process of Exhibit 147.

Eighth, the Kittle process of Exhibit 131.

Ninth, the Noad process of Exhibits 145 and 146.

Tenth, the Lang process of Exhibit 135.

Eleventh, the Turner process of Exhibit 132.

As I indicated in an earlier part of my testimony these tube cracking processes show a very wide variety of disclosures as to stocks, pressures and temperatures and details of construction.

The drawbacks which are essentially common to all of the prior art tube cracking processes are, first, that they cannot make very much gasoline per single pass without getting excessive gas losses. That is a fundamental drawback of that type of process where both the material being cracked and the products are kept in the cracking zone for the same length of time. If you are content with only ten per cent of gasoline per pass the gas losses are not so much higher, because you have not had enough gasoline in the system to decompose much of it, but if you want to make [fol. 915] anything like as much per pass as the bulk pressure distillation processes, the gas losses become many times as great as in bulk pressure distillation.

Furthermore, the tendency of coke to deposit toward the end of the cracking zone is another thing that makes it almost impossible to obtain a very large amount of gasoline per pass. Although tube-cracking processes will tolerate considerable coke per unit surface the stock is getting dirtier and it is generally getting hotter as it passes along through the tube so that the coke deposition tends to localize near the end, and that, as a rule, limits the cracking per pass.

If you are going to avoid frequent cleaning and if you are using stock that is comparable to the type of stock handled in bulk pressure distillation, the amount of gasoline that you can properly produce in the tube is limited to something like ten or twelve per cent per pass which, of course is a very serious handicap with these processes; though, if you operate it with low boiling vapors you can run and make more gasoline but still have the big drawback of high gas losses.

The third difficulty which is inherent in practically all prior art tube cracking processes is the fact that their available time is so short that they are forced to go to a fairly high temperature to bring about a reasonable amount of cracking and that high temperature, as I pointed out, gives a different type of product—a type of product differing much more from ordinary gasoline than does the product of bulk pressure distillation processes, being more highly unsaturated and having more tendency to produce gum, and containing more aromatic hyrocarbons, especially at the higher temperatures.

While the latter objection was one of the most serious reasons for the failure to use these very simple, and, in many cases, attractive tube cracking types of processes in the first ten years, or say up to within very recent years, it happens that that is one factor which is making some of these plain tube cracking processes for the last two or three years attain a degree of commercial success. It has been found that while these gasolines are quite different in character and do have a greater tendency to produce gum, and are harder to refine, that they do have a greater anti-knock value. Now that there has been created a market for an anti-knock gasoline at a premium figure, there are a number of smaller refiners who have found it possible to install simple tube cracking processes and tolerate the greater gas

losses and the lower amount of cracking per pass, and the [fol. 916] greater refining difficulties and the other undesirable features of the gasoline, in return for the fact that they can frequently charge a premium of two or three cents a gallon for such a product, which makes up largely for these extra costs. So that while none of them have attained a success anywhere approaching that of the bulk pressure distillation or the tube and tank processes developed by defendants, some of them have, within very recent years, attained a moderate degree of commercial success mainly for the reasons indicated.

The Master: Would the ten per cent in price, which is probably the difference in price, make up for the serious losses which you have enumerated?

The Witness: Opinions differ on that. Of course two cents a gallon to a refiner, who is only allowed ten or twelve cents a gallon at the refinery for his gasoline, looks a good deal bigger than it does on the selling price.

The Master: I see—I overlooked the difference in the price to the refiner and the price to the consumer.

Th Witness: There is a big marketing cost in there.

The Master: It is a 20 per cent instead of a 10 per cent increase.

The Witness: Between those two, perhaps.

Q. When did this demand for anti-knock fuels develop—when did they begin, or when did we begin to learn anything about that?

A. Except for a few isolated instances, it probably developed with the marketing of ethyl gas, which began, I believe, about two and a half years ago—that is gasoline treated with tetraethyl lead, to reduce the knocking tendency of ordinary gasoline.

Q. Now, in the original Burton patent, No. 1049667, appears the statement that a known method of treating the Burton stock, "involves subjecting the liquid to be treated to a temperature sufficiently high to secure so-called destructive distillation, and conducting the resulting vapors through a condenser by the way of a pipe, or conduit connecting it with the still, but having a loaded valve interposed in the conduit between the condenser and the still, to maintain pressure in the latter, of the vapors of distillation of the liquid. While this practice produces the desired

effect of lowering the boiling point of the liquid, the condensed product for reasons which I do not attempt to explain, is found to have been converted into distillates be- [fol. 917] longing to the objectionable ethylene type refered to."

Q. To what type of process do you understand this quotation in the Burton patent to allude?

A. I believe this quotation, without much question, alludes to the distillation of the Testelin and Renard type of operation, which was the only reference formally cited against the Burton patent.

In the Testelin and Renard patent the oil passes through a small tube, where it is vaporized, and into a larger tube, where the vapors are super-heated, and undoubtedly very extensively cracked, the tube being held at a red heat.

Apparently Burton refers to this larger tube rather inaccurately, as a still—that is, inaccurately in the sense that we have been using the term—and the operation of destructive distillation, is, of course, a term which any chemist would apply to a decomposition of this type, and does not, in any sense, imply a bulk pressure distillation, because the Testelin & Renard is plainly a tube cracking type of process, in which there is no large body of oil held at a cracking temperature for a long time, with the vapors of the desired product coming off substantially as fast as formed, but passes the entire products with apparently no liquid residue, through a series of successively hotter tubes, the last one of which is somewhat enlarged in size. The vapors then do pass through the loaded valve type of construction, which Burton refers to, and the product formed is of the highly unsaturated nature characteristic of such tube cracking processes.

It is true, the stock that Testelin & Renard refer to, is kerosene, but, of course, kerosene of those days, contained quite a bit of what we call gas oil and, furthermore, Noad, in a very similar type of tube cracking process, did use such gas oil. Of course, Noad was not under pressure, but, as I have stated, pressure is of comparatively minor importance in the tube cracking process.

The Master: Does the discussion in the File wrapper justify your inference that there was a differentiation from Testelin & Renard?

The Witness: Yes, it does. That is a further verification of the same opinion, in addition to the fact that Testelin & Renard was the only thing formally cited by the Patent Office, Humphreys, in his affidavit, compares the product of the Burton process with the product of an [fol. 918] operation which is essentially that type of hot tube cracking process—very much like that of Testelin & Renard. In other words, Humphreys was simply comparing the product produced by the Burton process, with that produced by the only other type of process which he knew, which used the same charging stock, except for the flash cracking processes, which also used gas oil.

Q. That Humphreys affidavit that you refer to is in that Burton File Wrapper, is it not?

A. Yes, sir, to which the Master referred.

Q. Now, so far as you know, was there any other type of process, excepting those tube and flash processes, which did use Burton's stock?

A. I have searched everything that I can find, published prior to 1912, and have found nothing outside of these tube and flash types of processes, which used Burton's type of charging stock.

Q. Now what have you to say as to the correctness of the theory in the Burton patent, No. 1049667, to the effect that condensation under pressure improves the quality of the distillate?

A. The theory set forth in the patent is correct, in part, but it does not tell the whole story.

As I pointed out, condensation under pressure, does not only increase the quantity, but it improves the quality of the distillate—particularly from the standpoint of these undesirable olefins, which Burton mentions, which we would possibly classify as gum forming constituents, and so forth.

The other difference between the Burton process and the prior art processes, using this type of stock, was, however, in the temperature of cracking, and Burton, apparently, did not realize that by working at a lower temperature in a bulk pressure distillation process, he was making, by that means also, a marked change, and improvement in the quality of his product. He, therefore, attributed all of the gain, or improvement in quality, to one of the two factors which

we now know combined to make that improvement, and if he knew of the other factor, he evidently did not mention it.

The Master: You mean, if he appreciated it?

The Witness: Yes, if he did appreciate it, he did not mention it. The number of variables involved in cracking, are, as I pointed out, very large, and it has been quite difficult for us to think of some reason for a change, without finding [fol. 919] that some other reason has as much, or more to do with it, than the one which we originally had in mind.

Q. Now, I wish you would please discuss the use of separating drums, in these eleven prior art tube cracking processes which you have mentioned.

A. Tube cracking processes can be divided into two general types—first, those which have little or no tar, or unvaporized liquid at the outlet of the cracking tube; and, second, those which have relatively large amounts of unvaporized material at the outlet.

The first type of process are characteristically the high temperature, low pressure type of processes, where even very heavy materials are vaporized, and in some of those, such as Testelin & Renard, and Lang, and Kornfeld—(and I am going to classify all of these that were cited)—the apparatus is so arranged that the tar is necessarily trapped in the cracking zone, and cannot escape, except, possibly, as entrained drops, and it, therefore, must be coked inside of the apparatus, and we have only vapors leaving and no need for separating drums.

The Master: Do you mean entrapped, as distinguished from the use of a trap?

The Witness: For example, in a great many of them the vapors pass down through a system of tubes, like this (indicating on blackboard) and are taken off vertically, from a relatively large chamber—an enlargement something of that kind (indicating) so that there is no good opportunity for the liquids to be carried along—they are simply left lying in the hottest part of the apparatus.

Q. Nothing can be got out of that kind of a system but vapors, and everything else has to be coked?

A. Of course it is conceivable that a little might be blown along the wall, but, in general, any tar is coked in the system.

Q. That type, you say, has no need for a separating drum?

A. That type has no need for a separation drum. If they do any separating they accomplish it in the hottest part of the system, and simply coke the tar in the system, which is, of course, one great difficulty with that type of process.

Some very similar processes, operating at low pressures, or high temperatures, or both, and, hence vaporizing most all their material, are the Noad and the Pielsticker vapor [fol. 920] phase processes, and Breitenlohner's process, and the Nikiforoff process.

In these last named processes it is conceivable that the tar might escape from the cracking zone, but there is no mention made of the fact. Practically all of it will be converted into coke, on account of being in contact with very hot tubes, and, so, again, these processes do not use, or require separating chambers.

In the remaining four processes, where there are considerable amounts of liquid, generally due to operating under quite high pressures, and, in general, not doing as much cracking, it is the uniform practice to separate the tar from the vapors, before condensing the vapors—which is of course a very logical procedure, to prevent contamination of the desired vapors, by the undesired heavy tar.

In the pressure processes this is sometimes done before, and sometimes after the release of pressure. The practice is not uniform in that respect.

In some cases, like in Kittle, the separating drums are in the heated part of the apparatus, and sometimes, as in Turner, the drums are not heated, and some condensation takes place.

In Schuchow & Gavrilow, the condensation, due to radiation, is accelerated by introducing the feed at that point, which cools and condenses still more of the material.

In the fourth case, that of Benton, the separating drum is fired on the bottom, and cooled on the sides and top, by exposure to the air, so that you may get almost any temperature conditions in that drum.

In none of the four is there the slightest indication of any chemical change taking place, or anything, in fact, except the mere separation of vapor from the liquid, which was

the function of the drums, and the purpose for which they were intended, and anyone skilled in the art would naturally design them for that purpose, which would mean, of course, small drums, allowing the very short time which is necessary for mere physical separation.

Of course they did have an indefinite amount of heating or cooling, which did not concern them particularly, because they were not interested in anything except separation.

Q. Now, you have already stated that the main obstacle to the production of commercial quantities of gasoline, by tube cracking processes, was the excessive production of [fol. 921] coke and gas, if it was attemtped to produce in a single pass, anything approaching the 25 to 40 per cent of gasoline attainable in bulk pressure distillation processes. How, if at all, has this obstacle been overcome in the practical art?

A. This obstacle has been overcome in the practical art, by the expedient of putting a large digestion vessel, or soaking drum—various terms are used—between the tube and the separating chamber, which were both old in the prior art.

It is possible in the drum, by proper control of conditions, to bring about further extensive cracking of the material held in the drum, in such a way that the coke does not settle on heating surfaces, and does not tend to clog up narrow passages such as those in the tubes—except, possibly, at the outlet from the drum, which can be, and frequently is, kept clear by special cleaning means.

And furthermore, by maintaining a fairly high liquid level in there, even though both the vapors and the liquid are held in the cracking zone for a long time, the rate at which the vapor passes through the cracking zone can be somewhat more rapid than that at which the liquid passes through, tending to reduce the excessive gas losses in this type of process.

That important contribution makes it possible to get very considerably more cracking, only a small additional expenditure of heat, and has been the thing which raised the tube cracking process, from being uncommercial, except in special cases, to being commercial.

Furthermore, by affording a longer time for cracking, on account of using these large drums under pressure, the

temperature can be lower and a product can be produced which, while not the same as that of bulk pressure distillation processes, is more similar to it than the characteristic, high temperature tube cracking product.

The Master: The thing that you are referring to is what I was shown over in Brooklyn, at the newly erected Cross plant,—that tank forty feet long and bored out to the diameter of forty inches?

The Witness: Yes, sir, that is the special drum, or the soaking drum of the Cross process. I believe they call it their reaction vessel.

The Master: That runs up to a pressure of fifteen thousand pounds to the square inch.

The Witness: Fifteen hundred pounds.

[fol. 922] The Master: I mean fifteen hundred pounds to the square inch.

The Witness: Occasionally but generally not as high as that. I believe they have operated in that vicinity.

Q. Now, after the liquid goes through that drum—the liquid and vapors—and further cracking, as you state happens, here—where does it then go?

A. It then goes to the characteristic separating chamber of the prior art—this separation generally being carried out at reduced pressure.

In other words, the drums do not have the separating functions that the prior art drums have, and they do have an entirely different function, for which the prior art drum is not suited.

The Master: I have forgotten whether there was a separating drum in connection with that, (Cross apparatus) or not.

The Witness: The base of the tower—either the entire tower, or the base of the tower, depending on the process under consideration, is the separation chamber.

Q. That operation would be just the same as in that Pielsticker type of still, with the tower, W, which you described yesterday, so far as the separation is concerned?

A. Yes, sir, if the tower, W, was put on the end of the pipe coil, as Pielsticker seems to indicate.

Q. So that those drums are an added feature, and not a substitute for anything?

A. That is correct.

The Master: "Drums" is so variously used, that if you call them a reaction chamber, or something of that kind, it would be more accurate, would it not?

The Witness: I think it would—I think that is a better name for it.

Q. Now, in what commercial processes has this principle of adding the reaction chamber—between the tube and the separating chamber of the prior art, been commercially embodied, and how do these processes differ among themselves?

A. This new principle has been embodied in, and is largely responsible for the commercial success of the Cross cracking process, developed by Dr. Cross and his associates, and now owned by the Gasoline Products Company, and also in the tube and tank process developed and owned by the Standard Oil Company of New Jersey.

The processes are the same in many of their general [fol. 923] principles. The Cross process characteristically operates at considerably higher pressure and attempts to produce less gasoline in the coil, and more gasoline in the drum, than does the tube and tank process.

The Cross process also uses a horizontal reaction chamber, whereas the tube and tank process uses one or more vertical chambers, which carry a somewhat higher liquid level than does the Cross process.

In the tube and tank process, the attempt is made to do as much cracking as possible in the coil, without having too frequent shut-downs for cleaning. That is, they believe in doing from eight to twelve per cent of the cracking in the coil, even if they do have to shut down more frequently for cleaning, and then getting up to twenty or twenty-five per cent of total cracking, by the time they leave the drum.

As a result, the tube and tank process generally runs from three to five days before cleaning, and the Cross process generally runs from seven to ten days between cleanings.

The Master: Let me refresh my recollection. As I recollect, there was a very small opening out of that reaction chamber, in comparison to the size of the chamber, and, perhaps, of the intakes. Is that difference in size the means of retaining the material in that tank for a definite length of time?

The Witness: No, there is a valve there, your Honor, which determines the rate of escape from that drum. There is a valve in that line—in the outlet line.

The Master: A valve which operates to relesae the material when it rises to a certain pressure—a pressure valve?

The Witness: It is a continuous process, of course, so that it is passing out continuously, and that valve is adjusted to maintain the desired pressure in the reaction vessel.

The Master: I was not speaking of the pressure, but of the quantity of material retained in the tank for a certain length of time.

The Witness: That depends on the pressure. You have a certain volume in your apparatus. Now, the amount you get in there depends on how much you compress it by putting pressure on it. It has a definite volume, and the only way you can change the amount of material in there is by the variation of the pressure.

The Master: That pressure is raised, in that reaction chamber, isn't it—generated there?

[fol. 924] The Witness: Well, it depends on how you look at it. The highest pressure in the system must be at the outlet of the pump. In other words, if you are going to keep forcing oil in, you must force it in at a higher pressure than prevails in the chamber. You could have just as much pressure as you wanted in there, without any heat or any cracking, just by boosting up the pump pressure, and throttling down the outlet, so that the oil could not get out.

Q. Is there any other particular feature in either of these processes, that you wish to call attention to?

A. In the tube and tank process the operation is sometimes modified by operating the two soaking drums in rotation. The oil is run into one soaking drum until it becomes fairly well filled up with coke, and then it is switched to the other one, while the first one is being cleaned out, so that as long as the pre-heating tubes do not accumulate too much carbon you can continue for a longer period of time, and you don't need to shut down the apparatus simply because the reaction chamber is filled with coke. That is the process known as switching drums.

Another interesting feature is that, in spite of the fact that these two processes are of the same general type—

that is, what we might call the tube and tank, or tube and reaction vessel type of process—they differ quite markedly in the relative amounts of coke, and the relative amounts of gas made, which is due to the fact that they have certain differences in their operating conditions, and that goes to indicate how sensitive cracking is to changes in operating conditions. That is, the Cross process, I believe in general, has around twice as much gas loss, as the tube and tank process, while the tube and tank process, has several times as much coke loss, as has the Cross process, due to variations in conditions.

Q. Has the tube and tank process any means for controlling the temperature in the tank, or reaction chamber?

A. Yes, sir, the tube and tank process, as developed by Howard and Loomis, and disclosed in a patent application still in the office, has a method for introducing a small amount of cold feed into the reaction vessel, to regulate the temperature therein, independent of the temperature of the coil outlet.

In other words, they find that by a proper design of the coil, they can run the coil quite hot, and do considerable cracking there, and then they may find that that temperature is too high to maintain in the drum—or, higher than they want to maintain in the drum, to get the desired products. Then they can bleed in more or less cold feed to regulate that temperature, independently.

Q. Now, I wish you would please describe the Cross process, as it is actually carried out, illustrating it with a sketch, if you have one.

Mr. Wiles: The drawing which is produced now, is offered in evidence as Defendants' "Exhibit No. 412."

The said drawing, so offered, was admitted in evidence, was marked "Exhibit No. 412," and the same is herewith returned.

[fol. 924a] Mr. Royce: If you don't mind, I would like to have the witness state whether he prepared this diagram, or whether it was prepared under his direction. It is simply marked "Cross".

Q. You may answer that.

A. The status of this drawing is exactly the same as that of the other drawings, with regard to these various pro-

cesses—this being a diagrammatic representation of the essential features of the Cross process, as ordinarily carried out.

The Master: It makes no difference who prepared it. He simply utilizes it as an illustration.

Mr. Royce: But if there are errors in it, and the witness——

The Master: It makes no difference how many errors there may be in it. If you wanted to contradict his testimony you could show it by showing that he used a diagram that did not correspond to the process, at all—just the same as if you wanted to argue a case before a Court, and you made a lot of charts, and you had them on a blackboard, or had somebody else prepare them, and set them up in the Court Room, and utilized them in your argument—they are merely illustrative, and if they don't show what you say they show, the Court is apt to find that out at some time.

You may answer the question.

A. The Cross process is the simplest and the cheapest, per unit capacity, of any of the continuous processes, and gives excellent results, especially where it is desired to keep the investment cost in plant as low as possible.

The feed, under a pressure, which differs in different plants—but is characteristically around 750 pounds—comes from the pump as indicated in the upper right hand corner, [fol. 925] and passes through a dephlegmator in the top of the bubble tower, thence through the line to the pre-heater. You notice that the arrangement of pipe is somewhat different here, so that the oil in the lower part of the furnace, flows parallel with the furnace gases—I mean, in a general direction upwards, in such a way that the hottest gases do not strike the oil when it is at the highest temperature, that tending to minimize the danger of excessive carbon deposits on the lower row of tubes. It then passes into the reaction chamber at a temperature of about 850 degrees, and at a pressure of around 700 pounds.

The pressure in this device is controlled by a pressure release valve, and in case of danger from clogging, there is usually a special cleaning mechanism to keep this valve and outlet line free from deposits of coke, which might shut off the chamber.

Beyond the reaction chamber is the separating drum, (operated at pressures of generally ten to twenty pounds), from the bottom of which the heavy tar, suitable for fuel oil, is withdrawn, and from the top of which vapors pass to the base of the bubble tower, which is similarly, generally under ten to twenty pounds pressure.

The vapors rise through this bubble tower, and the parts which are heavier than gasoline, are condensed out by the heat exchanger, or dephlegmator, and are drawn off from the bottom of the tower, after having been stripped of their light ends, by the hot vapors ascending through the plates.

This cycle stock is not returned directly to the process, but is cooled and put in with the feed stock.

The desired vapors pass over and are condensed in the condenser, and go to the gas separating drum, where the separation is effected at comparatively low pressures. The saving which is accomplished by such separation is less than might be realized by separation at somewhat higher pressure.

Q. What about the velocity in the tubes of this process, and its effect?

A. The velocity in the tubes of this process is purposely kept very high, in order to minimize carbon deposits, and the general effort is to keep the amount of cracking in the tubes, as small as possible.

Q. What effect has the fact that the drum is not heated, on the carbon trouble?

A. The drum can get almost half full of carbon without serious interference with the operation, and it is character [fol. 926] istic of the operation, to allow that chamber to fill up pretty well with carbon, before it is opened and cleaned out.

Q. I wish you would describe, in the same way, the tube and tank process, as operated by the Standard Oil Company of New Jersey.

Mr. Wiles: And the drawing which is now produced and handed to the witness, of the tube and tank process, is offered in evidence as Defendants' "Exhibit No. 413."

The said drawing illustrating the tube and tank process, so offered, was admitted in evidence, was marked "Exhibit No. 413," and the same is herewith returned.

Q. Now, you may answer the question.

A. The status of this drawing is the same as the others, in illustrating the essential features of the tube and tank cracking process, as carried out by the Standard Oil Company of New Jersey.

The feed from the pump, at a pressure of around 400 pounds, passes through the dephlegmator in the upper part of the bubble tower, and thence to the heating coil, which is arranged in a somewhat different manner from the others, and also has, in part, the principle of not exposing the hottest oil to the hottest furnace gases, tending to minimize the localization of carbon.

As I said, the effort in operating this process is to do considerably more cracking in the coil, than is done in the other types.

The Master: That is, in the Cross type?

The Witness: Yes, in the Cross type. The temperature, leaving this pre-heater, is around 850 degrees, and the pressure is around 350 pounds. In this case the feed enters the digestion vessel near the bottom, and a small amount of cold feed may also be introduced, as necessary to control the temperature of this vessel.

The liquid level is ordinarily maintained rather high, although not always quite as high as here indicated, and both the vapors and liquids pass out together through the pressure release valve, at a temperature of around 785 degrees Fahrenheit.

The Master: It is marked 700, here.

The Witness: No, that refers to the temperature in the bubble tower 785 is the temperature at which they leave—or, at which they come to the pressure release valve. They then discharge into the separating chamber, which is car- [fol. 927] ried at a pressure of about 60 pounds, and a temperature around 700 degrees, the heavy tar passing out at the bottom, and going through a tar cooler, and being generally sold as fuel oil, while the vapors rise past a collecting pan, into the upper part of the tower, where the heavy cycle stock, and the heavier part of the distillate is condensed and returned to the pan, and taken out through a cooler, this being the cycle stock which is generally put back with the feed—sometimes put back fairly hot.

The vapors passing off from the top of this tower, are condensed in the main condenser, and the gas is separated from the distillate in a drum, which is held at about 50 pounds pressure, in order to decrease the gas losses.

As I pointed out, they use vertical drums, either alternately, or sometimes simultaneously.

The Master: Now, wait a minute. When you say "drums" there, you mean the "Soaker" or digester?

The Witness: I mean the two reaction vessels.

The Master: They are marked "Soaker" or digester here.

The Witness. They are operated either simultaneously or alternately.

The Master: What distinction is there between this and the Cross process aside from the pressure which, in one instance is 750 pounds, and in the other is 400 pounds, and that the reaction chamber is vertical instead of horizontal?

The Witness: There are, of course, some differences in temperatures.

The Master: Yes, there is a difference in temperatures.

The Witness: And there are differences in the temperature control, being in one case by cold feed. There is also considerable difference in the pressure at which the bubble tower is operated, but the processes are, by and large, quite similar, save for a different choice of preferred operating conditions. They are both of the tube and tank type.

The Master: The steps are just the same?

The Witness: Substantially so, yes; substantially identical in steps.

Mr. Wiles: We have already brought out, I believe, and this may have a bearing on some of the interference matters that we will have to allude to later, Cross does practically no cracking in the tube, or as little as possible. In the tube and tank they do about as much as they can without serious trouble. That is correct, isn't it?

The Witness: Yes. That is probably the outstanding difference in the way they are operated.

A. In addition to the four practical and substantially independent lines of cracking developments carried out by the research and development staffs of the Standard Oil Company of Indiana, the Standard Oil Company of New Jersey and the Texas Company and by Dr. Cross and his associates, there were several independent inventors who, with

much less close contact with the practical art, worked along on an experimental scale trying out various methods of cracking. A number of these working at an early date—I have in mind particularly Palmer and Adams and Ellis—were the first to disclose certain features which were later found to be of very great value and important in the practical art.

In many cases these features of great practical value were associated with others of less practical value, or which would not prove to be economical or entirely satisfactory on a commercial scale. Nevertheless, they were the first to show these valuable features, and they described them adequately in patents which eventually issued. Of course, on account of the early dates and the fact that they were not directly tied up with any one of these four independent developments they greatly complicated the patent situation.

The essential feature of Palmer was in the disclosure that is was possible to bring about cracking in the entire ab- [fol. 928] sence of distillation, by merely sealing material up in a vessel and exposing it to the high temperature. You may recall my pointing out, in discussing the prior art, the prevalence of the wild notions of the relation between distillation and cracking and between dropping back and radiation from hot surfaces, and how confused the art was. Palmer was really the first man who pointed out that cracking could and did take place in entire absence of distillation and that you could simply seal up material, and heat it up and get very considerable decomposition.

In the reference which were cited against Palmer distillation always accompanies decomposition. The references cited, as I recall it, were Lobry De Bruyn, and Jacunski, both of which worked as you recall, in bent glass tubes which were sealed; but while these were sealed from contact with the atmosphere, one side was kept hot and the other side relatively cold and distillation always did take place. They were carrying out a pressure distillation process in a closed chamber and the cold leg was always held at a temperature below a cracking temperature; so that, as plaintiff's expert substantially admitted on cross-examination, page 1008, distillation was substantially simultaneous with cracking in these processes. That misses the whole point of the Palmer idea.

Q. Now, I want you to state what bearing this patent has on the Burton process?

A. As I have already pointed out, one of the important steps in the Burton process is the digestion step in which the material is heated up to a cracking temperature without permitting distillation to take place, and I have also pointed out that during that period a marked change takes place in the character of the stock in the still, which affects the ultimate running rate, and, furthermore, that the rate of firing is deliberately slackened some little time before the still starts to run, in order to prolong that digestion step and accentuate the change.

Now, it is true that the amount of change which takes place in that process is very much less than the amount of change which Palmer is describing, and that simply raises the question of equivalence and all I can do is to state the facts as I have done.

[fol. 929] During digestion in the Burton process the initial boiling point of the stock is lowered, well, probably 50 to 75 degrees on the average.

The average boiling point of at least half of the stock in the still would be lower than before the digestion period. As I pointed out, if we do not slow down the firing in this way it makes a distinct difference in the method of operation of the still and in the steadiness with which it operates.

Plaintiff's expert said that in that sence, that is the digestion which Burton got in his process, anyone who heated to a cracking temperature would get a little cracking before distillation began. I presume that from a theoretical standpoint, that could not be denied but the fact of the matter is the Laing who, I believe, was the one specifically mentioned, made no disclosure whatever of slackening off the fires some little time before running began, which, in the operation of the Burton process, is necessary to bring about the desired substantial change in the character of the stock in the still.

Furthermore, Laing, working with a relatively much smaller still, therefore with more heating surface per unit of volume in the still, could heat up much more rapidly than Burton would and would therefore have very much less change than Burton would obtain even if Burton heated directly to a running temperature. We find that in small scale stills to produce this same digestion action and the

same change in the stock in the still before running, which we want in order to have them start to run at the proper operating temperature, we must slacken off the fires for a considerably longer time, an hour or two, before running starts if we are to accomplish the desired purpose. And in the absence of any such disclosure by Laing we must assume that any change which took place in his case before distillation began was entirely negligible.

Q. That difference between small stills and large ones is simply due to the greater heating surface necessarily present in the small still, is it not?

A. Compared to the volume in the still, yes.

Q. So that if you fired up normally a small still will rise in temperature so rapidly, that you will begin distillation before anything happens?

A. Before any substantial change takes place.

Q. Now, if you fire normally in a laboratory sized still how high can you run Burton's stock before distillation actually begins?

A. You run it up without any difficulty to 750 degrees before distillation begins, and then have to work your way back to 725 with the concomitant of poor distillate during that starting period.

Q. That is due to the absence of prior digestion before you begin to run?

A. Yes.

Q. Now, what bearing, if any, does the Palmer patent have upon any other of the defendants' processes, that is the Texas, tube and tank, and the Cross?

A. In the narrower sense of an entirely stationary body of oil held without distillation, Palmer is of course found only in Burton, but in its broader aspects of accomplishing substantial change in a large, relatively quiescent body of oil without permitting distillation to take place, that same action is present in the reaction vessels of the tube and tank and of the Cross processes which I have just described.

Q. Now, if you give the Palmer invention that broader aspect, are there any additional references which would bear on it which were not cited against it by the plaintiff?

[fol. 930] A. We would, in fairness, have to consider Benton's disclosure, who did disclose the use in his cracking coil of such high pressures as to substantially prevent vaporization, but Benton, as I pointed out previously dis-

closes that by his method of operation he converts gasoline into kerosene.

If that be true his operation is clearly entirely different, and the change which he produced is clearly entirely different from that of Palmer who made lighter oils by his digestion process.

If, on the contrary, we assume that Benton did not accomplish what he said he did, but actually made lighter oils, as he probably did, then he simply is mis-stating the facts of what did take place, and we would then be able to disregard him as a pertinent reference in respect to that change.

Furthermore, even apart from those points, the type of digestion which Benton had in a narrow, rapidly flowing stream was certainly further away from Palmer's large, quiescent vessels than the relatively large reaction vessels of the Cross and tube and tank processes.

The Master: Do you regard these theories of what takes place as stated by Palmer as correct?

The Witness: Palmer?

The Master: Yes.

The Witness: Yes. It is the Benton theories which I do not regard as correct. The Benton statements of what happens, not so much the theories as to how it happens; but Benton misstates, in my opinion, the actual facts of what happens. If he is right in his facts then his operation is quite different from that of Palmer.

The Master: In other words, you are put to it here to try to save Palmer and at the same time prevent Palmer from anticipating Cross and the tube and tank processes?

The Witness: I should not consider it anticipated. It would cover one feature that they are using, but they have many features of novelty in their process. It would not anticipate the novelty of the Cross process.

Q. Comparing Palmer with the tube and tank and Cross, Palmer is devoid, is he not, of the tube entirely?

A. Yes, and of many other important features.

Q. And of the flow?

A. Yes.

[fol. 931] Q. Now, Mr. Wilson, referring to the Adams process and apparatus patents, Nos. 1320354 and 1370263, plaintiff's expert asserted that while these patents described a vapor phase process they claimed a liquid phase

process. Now, what is your understanding as to the physical condition in these processes,——

The Master: Now, what are you talking about?

Mr. Wiles: Adams.

The Master: Adams; oh, yes.

The Witness: What are the numbers again?

Mr. Wiles: Nos. 1,320,354 and 1327268. The first is the apparatus and the second is the process.

A. It seems to me that no valid question whatever can be raised as to the presence in the domes of Adams of a substantial body of liquid. He describes this body of liquid in his specifications; he shows clearly a liquid level in the domes of his patents. Do you have a drawing that shows the domes?

The Master: I suppose this dotted line is your liquid level (indicating)?

The Witness: Yes.

The Master: In the chamber E.

The Witness: Referring, for example, to figure 2 of Patent No. 1320,354. In these and other drawings, he clearly shows a liquid level in the drums, and shows the gauge glasses on the side of a column connected therewith, in this drawing being the column K to the right of the vapor dome, for permitting the observation of the liquid levels, which column would be useless save for that purpose. He also shows and describes in these patents the overflowing of liquid from a pipe placed at the liquid level for the purpose of carrying away liquid.

As I say, there can be no question whatever but that Adams clearly discloses the presence of a substantial body of liquid in the domes of his apparatus.

Q. What do you understand is the basis for the error of plaintiff's expert in concluding that these processes must have been vapor phase?

A. I think the basis for his error must have lain in assuming that Adams' frequent references to the temperatures of his heating surfaces bore some necessary or close relationship to the temperature of the body of oil around them. Whereas as a matter of fact, in any case where there is a flowing stream or a body of oil in contact with the heating surface there may be almost any relationship at all

and no necessary or close relationship between the temperature of the heating surfaces and the temperature to which the oil is heated.

The Master: The body of the oil?
The Witness: The body of the oil is heated.

Q. Now, do any of the prior art references show any such disparity between the temperatures of the oil and of the heating surfaces?

A. Yes. Kittle shows a difference, as I recall it, between his flowing stream of oil through a coil immersed in a lead bath and the surrounding lead bath of 360 degrees Fahrenheit, approximately.

The Master: A diffeerence?
The Witness: A difference.

A. (continued). Of course, such differences are common experience with all of us. For example, in a pressure still we may get a red hot bottom with the still contents at 750° or lower.

Q. Can you illustrate this lack of necessary relationship?

A. I think I can (if this experiment does not follow the course of many lecture experiments!) I would like to just illustrate the entire possibility of having a red hot electrode immersed in a very cold body of oil, by showing this arrangement here in which I have a small piece of tungsten wire between two electrodes, connected across a six-volt storage battery. Of course, this illustrates it on a very small scale (illustrating with apparatus).

The Master: What have you in the glass there?
[fol. 932] The Witness: I have some white mineral oil, commonly known as Nujol. (Witness makes the wire red hot.)

And if you care to feel the beaker you notice that it is still quite cool.

Q. Now, in this Adams process there was a considerable flow of oil past a hot surface, was there not?

A. You, definite and fairly rapid circulation past the heated surfaces.

Q. And that such flow could be maintained precisely as this experiment illustrated, even over red hot surfaces without the body of oil all evaporating?

A. The temperature that is maintained in the body of oil depends on the relation between the amount of heating it gets and the amount of cooling it gets from the surroundings—if it were well lagged it would probably go to the maximum temperature, or to the boiling point of the oil under the particular pressures prevailing. Of course, undoubtedly the electrodes are locally surrounded, by an insulating vapor of film. That is, the red hot electrode forms a film of oil vapor around itself—that is what permits it to get so hot. You may have noticed there that the wire showed no signs of redness until suddenly it burst into redness. And what happened simultaneously, as close observation indicates, is that there was a film of vapor formed. Of course, in that film of vapor the part that is closest to the electrode gets to quite a high temperature, and such references as Adams made, I believe, in his file wrapper to very high temperatures for any part of his oil must have referred to the very localized film in which undoubtedly some of his cracking took place in immediate contact with his electrode.

Of course, in the externally fired apparatus of the other patent, the red hot surfaces may have been due to an insulating layer of coke on the inside as well as possibly to an insulating layer of vapor under some circumstances.

Q. Now, Mr. Wilson, the record does not show just what happened in this experiment. It was very graphic to all of us, but I wish you would state, just briefly for the record, what you did.

A. I imposed almost the full six volts of the battery across a piece of tungsten wire a little less than an inch in length.

Q. Immersed in the oil?

A. Immersed in a beaker of oil about two and a half inches in diameter and about two inches below the surface. [fol. 933] As the current was increased the filament suddenly become incandescent, the current reading being in the neighborhood of 20 amperes. The electrode remained red hot with evidences of rather violent agitation and a high-pitched gugling noise emanating from the vicinity of the beaker.

The Master: And the giving off of some vapor.

The Witness: And the giving off of some vapor.

A. (continued). At the close of the experiment the beaker was still cold, the wire heater having only been operated for a matter of possibly a half a minute.

It simply illustrates the fact that there is no necessary relation whatever between the temperature of the heating surface and the temperature of the material to be heated. Of course, the familiar example of a drop of water on a red hot stove is another example of the same thing; you cannot heat the water above 212. What you have is the insulating film of vapor which keeps the body of water away from direct contact with the stove.

Q. Plaintiff's expert also stated——

The Master: What this showed is that in order to heat that body of oil you have got to confine it next to the heating surfaces for a sufficient length of time for the body of oil to absorb the heat?

The Witness: That is one thing that it shows. The particular thing we wished to emphasize was that the mere fact that the heating surfaces were at maybe 1500 or 2000 degrees F., was no proof that the oil was at any such temperature, or that you could not have liquid oil there.

Q. Plaintiff's expert also stated that the pressure in the Adams process patent, No. 1,327,263 must necessarily be extremely low because Adams suggested graphite as a possible material for his heat resisting tube; plaintiff's expert claimed that a graphite tube was incapable of withstanding high pressure. On cross-examination he stated that he based his opinion upon the work which he had done in attempting to maintain a vacuum in a graphite receptacle, which he said he found impossible because of leaks through the somewhat porous material.

Tell us whether there is any real parallelism between maintaining pressure and maintaining vacuum in receptacles of this character?

A. No, there is an almost complete lack of parallelism between those two problems, so far as the porosity of the [fol. 934] graphite is concerned, which was the only point raised, it is true that it would make it difficult to maintain a good vacuum in an apparatus made of graphite, because we are concerned with air leakage into the apparatus, and air, being a very fluid medium, leaks in through into very tiny holes and in quantities sufficient to spoil the vacuum.

Furthermore, there is nothing in this case which would tend to clog up the pores.

On the other hand, if you put pressure on an apparatus and have oil on the inside, in the first place, we have a much less fluid medium so that the amount of leakage outward would be much less, and such leakage outward as did occur, while annoying, would not be particularly serious.

Furthermore, and more important than that, is the fact that if this graphite is at anything like a red heat or even at a lower cracking temperature, the oil coming into it would almost immediately coke and clog up such pores. So, as I say, as far as the porosity of the graphite is concerned, which was the only point raised, there is no comparison between the vacuum operations which plaintiff's expert described, and the possibility of maintaining pressure.

Now, you just mentioned this tendency of oil to coke up small leaks in the cracking apparatus, of which plaintiff's expert was evidently ignorant. Now, I want to know whether that was well known to those skilled in the art?

A. Yes, that is a very well known phenomenon which saved many of the early cracking operations from failure. If the leakage had continued at the rate at which it customarily began in the early stills, I think there is no doubt but that we would have been unable to accomplish any cracking. That coking up tendency is very familiar, and is the one place where coke formation does help us out in the cracking industry.

Q. Your small leaks are self-sealing?

A. Yes, if they are below the liquid level, particularly.

Q. In view of these facts, tell us your opinion as to the possibility of maintaining substantial pressures in an apparatus such as Adams disclosed?

A. I do not know enough about the exact strength of graphite prepared in different ways or compounded in different ways, to calculate the precise pressures which could be maintained; but I do know that while steel loses strength very rapidly as a red heat is approached, graphite maintains a much larger proportion of its strength at high temperatures, particularly at very high temperatures.

If we had a small diameter, thick-walled graphite vessel, I see absolutely no reason why it would not be perfectly

possible to maintain substantial pressures, even at quite high temperatures such as Adams was considering. Furthermore, I note on page 2020 of the book of exhibits in this Adams patent No. 1327263, beginning with line 9, the following statement by Adams:—"An oil containing and cracking chamber may be constructed of various heat-enduring metals, alloys, carbons, graphites, graphitic carbon and other materials known to withstand a constant or varying temperature of high intensity, but as the structural character of such a part does not relate specifically to a process invention in this process disclosure——"

It will be unnecessary to develop it further than this. Within the scope of what he suggests there in metals and alloys, there certainly would be no difficulty whatever in finding materials capable of standing quite high pressures. [fol. 935] As far as I can find, Adams was the first man who, in a bulk pressure distillation process for making gasoline, disclosed some seven important features. These features were

(a) Indirect heating of a bulk supply of oil in a continuous process of this type.

(b) Condensation of the gasoline distillate and separation of the gas therefrom, under still pressure.

(c) Feeding fresh stock and drawing the pressure tar continuously.

(d) The application of heat to the intermediate portion of a vertical cracking still, below the liquid level.

(e) The use of agitators and carbon scrapers on the heated section of a liquid vertical cracking still, and drawing off the tar and scrapings at the bottom.

(f) The use of a plurality of vertical cracking stills, operated in series, with a common vapor line.

(g) The separate condensation and recracking of that part of the cracked vapors which is heavier than gasoline.

The Master: Do you mean to say that Adams disclosed all that?

The Witness: Adams disclosed all of those features, and, as far as I can find, he was the first to disclose those fea-

tures in a bulk pressure distillation process for making gasoline.

The Master: I recognize some, but not all of them.

The Witness: Well, they are all quite clearly disclosed in various and sundry patents.

The Master: I was just thinking of the first electrically heated patent.

Mr. Wiles: For instance, the battery is another patent that has not been very thoroughly considered here, and I may say that it is very much easier to handle these as features, than it is as patents.

The Master: Well, your grouping is a good one, because it groups the developments of a particular inventor along certain lines.

[fol. 935½] Q. The first of these important features which you have attributed to Adams, is an indirect heating of a bulk supply of oil in a continuous pressure cracking process. Now, state in which of the defendants' practical processes this is employed?

A. Well, this feature of indirect heating of a bulk supply of oil in a continuous pressure cracking process, is disclosed by Adams as a part of a bulk pressure distillation process. The same feature, in a broad sense, is as I have already pointed out, also useful in the tube and tank type of process, including the Cross process.

The step is employed by the Holmes-Manley process, in a manner very closely analogous to that in which Adams disclosed it, although, as I have pointed out, the stills are lightly fired, to minimize radiation losses, and supply a very small amount of heat—the main bulk of the heat, however, coming from the heating of the restricted stream before it discharges into the bulk supply.

Considering Adams' invention in a somewhat broader sense, it is also found in the Clark tube still, where a large bulk supply of oil is kept hot by the recirculation of a confined stream.

And, also, in this broader sense, it is present in the Cross, and the tube and tank operation, where there are large unfired reaction vessels which are kept heated by the stream which was previously heated in a confined and restricted passageway.

Of course the Clark tube stills are not continuous. They are only semi-continuous, in accordance with the definition I have given, whereas, the other operations are continuous, as Adams, also, was.

Of course the essential value of this feature, as I have pointed out before, consists in putting in the heat in a restricted passageway, where the velocities are comparatively high, and then carrying the material into a large [fol. 936] bulk supply of oil, either not heated at all, or only lightly heated, where carbon is deposited without making hot bottom troubles, which are otherwise generally found in bulk distillation processes, where you crack to an extent sufficient to make gasoline.

Plaintiff's expert did not consider this feature independently in discussing the Adams patent and, therefore, we cannot be sure, exactly, what he might have cited.

But it would seem impossible to cite anything more than the DeClercq and Pielsticker of the four tube cracking processes, which showed separating drums—these being the only ones with structures even remotely resembling Adams in respect to the method of introducing heat.

With regard to DeClercq, I have already discussed the DeClercq reference in connection with the Clark reference, and having pointed out the failure of DeClercq to disclose pressure, or cracking, and the unsuitability of his apparatus for either purpose, thus clearly distinguishing him from Adams. In addition, we have the point that Adams was a truly continuous process, while DeClercq was a batch process, being, in that respect, closer to Clark, who had a semi-continuous process, than he is to Adams.

So, considered in the narrower aspect of continuous operation, there is no question but what this feature is not even remotely approached by DeClercq.

I have also discussed the application of heat to the drum in the Pielsticker process, and shown that Pielsticker's disclosure could only be fairly interpreted as applying a great deal of heat to the large drum, and that he, presumably did not heat to a cracking temperature in his coil, but only went to around 400 degrees, and then the rest of the way by the heat put into the drum, assuming, roughly, equal firing in the two sections, as Plaintiff's expert said he considered reasonable, in view of the similar furnace settings. In that

case we would be very far from having the advantage of the indirect heating of the bulk supply of oil, because the coke deposition in the bottom of the still would give the hot bottom troubles.

And Pielsticker also fail to have Adams' feature of heating to a cracking temperature, in the restricted passageway, before it discharges into the main bulk of oil.

With regard to the four tube cracking processes that have separating drums for separating the tar from the vapor, I have already pointed out that in not one of those—which [fol. 936½] include Kittle, Benton, Schuchow & Gavrilow and Turner is there any relatively large body of oil which is kept heated by the discharge into it of a stream of oil which has been heated in a restricted passage.

Q. The second of the seven features of Adams that you mentioned is condensation of the distillate and separation of the gas therefrom under still pressure in a gasoline-making process. Please state in which of the defendants' processes you find this.

[fol. 937] A. In the strict sense of condensing under full still pressure and separating the gas therefrom under full still pressure, Burton is the only one of defendant's processes which employs it, and Burton obtains, as I have already pointed out, the utility, unique to a gasoline-making process, of increasing the quantity and improving the quality of the distillate.

The same identical utility is, however, obtained in a slightly modified way in the Holmes-Manley process, where the separation is carried out not at full still pressure but at a somewhat lower pressure like that at which Burton operated and presumably is nearly in the range at which Adams was operating, because we find that the maximum saving in quantity of gasoline is to be made by separating the gas at pressures generally between seventy and one hundred pounds.

As I pointed out, the feature is used strictly in the sense noted in the Burton process and is used in such a way as to obtain the identical utility by reducing the pressure somewhat from full still pressure in the Holmes-Manley process, thus separating the gas at a pressure of around eighty pounds, which in this process gives the maximum advantage in separating the gas from the distillate under pressure.

As I point out, while higher pressures will still further reduce the volume of gas carried away it increases the amount of gas which dissolves in the liquid and must be separated from it at atmospheric pressure at a later time. The advantage is also obtained to a still somewhat smaller extent in the tube and tank process, and to a slight extent in the Cross process, these separating their gas at progressively lower pressures and not at the pressures of their drums. But essentially the same utility is realized to varying extents in all those processes.

I have already pointed out that this utility is not present in any of the prior art bulk pressure distillation processes. The only one who might conceivably have had it, as I pointed out, was Dewar & Redwood, but on account of pumping in additional gas most of the time he, on the average, lost more gas than—or lost more distillate than he gained. And, furthermore, even if he had made a microscopic gain the utility was absent in his case because the gain did not improve the quality of his kerosene, but the [fol. 938] saving was in materials which would have to be removed from his desired product in order to make it marketable.

Plaintiff's expert also cited the Lang Patent, United States 954,575, as anticipating this feature of Adams. Apart from the fact that Lang is not, of course, in any sense a bulk pressure distillation process, but is a tube cracking process, and that his process would not have been practical as a cracking process on account of excessive deposition of coke right in the hottest part of his apparatus, especially starting as he did, with crude oil and allowing the unvaporized residuum to collect in a chamber right over the fire, we have the additional fact, which was apparently overlooked by plaintiff's expert, that Lang was really not trying to make gasoline; thus he indicates in the second paragraph of his specification that he wanted a hydrocarbon of such high volatility that it would continue to vaporize at a relatively low temperature, such as that of snow, in accordance with his snow-melting Patent No. 863,855.

Referring to that patent, a copy of which I have here——

Mr. Wiles: I offer it as Defendant's Exhibit 414.

(The said patent so offered, was admitted and read in evidence, was marked "Defendants' Exhibit No. 414" and the same is herewith returned.)

A. The process which Lang describes in here is a process of removing snow from manhole covers or switches or things of that kind. I understand that Lang was at this time, and has been for many years, the superintendent of the South Station in Boston, and one of his grievous problems was getting the snow dug out of switches in those tremendous yards, and he conceived the idea of getting a very volatile hydrocarbon which could be poured on there and lighted and thus melt the snow away in a short time.

He indicates, as I said, in line 2' of this patent, that gasoline is absolutely useless for this purpose of removing snow from switches, because it does not volatilize rapidly enough.

It will be noted in the next paragraph that the product which he prefers is described in line 34,—as "liquefied gas, a fluid hydrocarbon product obtained from the high compression of Pintsch gas and having a degree of volatility much higher than gasoline, which will freely volatilize at a temperature of 32 degrees Fahrenheit or under."

This rather unique product which he refers to as "liquefied gas" can, of course, only be obtained by condensation under pressure. Pressure is a means of liquefying a gas, of course as we well know. Apparently, it must be kept under fairly high pressure and must be quite different from gasoline, because it must apparently boil at or around thirty-two degrees Fahrenheit in order to volati-ze freely after it sinks down in the snow, as he describes.

Such liquid gas or very wild material, which would tend to boil out of gasoline at ordinary temperatures is, of course, not in any sense a product desired in gasoline, and if anyone skilled in the art was told that by condensing under pressure he would recover a product of that kind, he would promptly decide that the thing to do was to follow the prior art and condense at atmospheric pressure, not taking the chance of getting a product of that nature.

In other words, pressure condensation in Lang could not possibly have the same function as in Adams or in Burton, Adams having the earlier application date.

Q. Now, the third feature that you mentioned as attributable to Adams is the feeding and drawing of tar in this type of process generally. Please state in which of the four defendants' processes you find it?

[fol. 939] A. The step of feeding and drawing of tar in a bulk pressure distillation process is found, as Adams disclosed it, only in the Holmes-Manley process and the continuous process at Whiting. However, the drawing of tar from the Clark type of tube stills has frequently been considered, and is even today being considered very seriously, and it is quite possible that under certain circumstances it would be found highly desirable to do as Adams suggests; that is, draw tar continuously from this type of process.

Q. Was feeding and drawing tar old in process for making kerosene by bulk pressure distillation?

A. Yes, it was. It was described in processes such as Barbet and Pielsticker, for example.

Q. State whether drawing tar in a kerosene-making process has the same function and utility as in a gasoline-making process?

A. No, the function or utility of drawing tar in a kerosene-making process is quite different. In a process such as a light cracking to make kerosene, the tar is frequently changed so little that it is still suitable for other uses. Thus Barbet indicates that his bottom is suitable for pressing to get out the paraffin, and Pielsticker would recover a petroleum residuum which would be very little changed, almost as valuable as the product of an ordinary skimming operation, as a residuum. In other words, many of these processes doing light cracking for making kerosene had a specific and important purpose in withdrawing their tar, because it was still valuable if they got it out soon enough. On the other hand, if they left it in for a little while longer, or did the more severe cracking necessary to make gasoline, the tar became almost worthless for any other purpose except for further cracking to make gasoline, and naturally the tendency was as long as it was hot in the still at a cracking temperature, to crack it down just as far as possible, and one *one* in the prior art, apparently, had any purpose for withdrawing it.

[fol. 940] Even Burton did not realize there was any utility in drawing tar; nor did Clark in his tube still. They

all felt the thing to do in a gasoline-making process was to leave the tar in just as long as you could. So Adams was apparently the first to appreciate the real utility of this step in a gasoline-making process, namely, that it permits longer runs if you withdraw the tar some time before it gets too dirty, this utility being quite different from that in processes cracking for kerosene.

Q. In one case you withdraw it to use the tar and in the other to avoid its harmful reaction on the process?

A. Yes.

Q. Do you find any references prior to Adams which disclose a step of feeding and drawing tar in a gasoline-making bulk pressure distillation process?

A. No. I do not.

Q. The next feature of Adams which you enumerated consisted in applying heat to the intermediate portion of a vertical still, below the liquid level, in a bulk pressure distillation process. State in which of the defendants' practical processes you find this feature and state its value there.

A. This feature is found only in the Holmes-Manley type of process and in the continuous stills at Whiting, which are very similar. Its value is very great, as I pointed out, it being one of the two things which is primarily responsible for the success of the Holmes-Manley expedient of putting heat in small amounts into the bulk supplly of oil while it is held at a cracking temperature. That was found necessary, in order to put nothing but clean stock through the tubes, as you will recall, and, as I said, at first sight, putting some heat into the bulk supply might seem to be a step backward, since we made our first gain by taking it away. But by virtue of side firing below the liquid level, which permits the walls to be kept cooled by the circulating body of oil inside the still, it is possible to carry out light firing successfully, particularly when combined with the later feature of the carbon scraper.

Q. When you use the term "Holmes-Manley" in this part of the testimony, you include the Whiting continuous stills?

A. Yes, unless I specifically distinguish them I mean to include the Whiting continuous stills.

Q. Is this utility of side firing that you have spoken of present in atmospheric processes as well?

[fol. 941] A. No, it is not. To do side firing requires a considerable sacrifice. It makes a more expensive furnace construction to have a vertical still side fired. Whereas with the horizontal still the gases rise and hug the bottom of the still and give good heat transfer, in a vertical still the tendency is to rise straight up away from the still, and it is hard to get the gases into contact with the still and put any considerable amount of heat in. To put a blanket of hot gases around a vertical still is not a bad expedient for a very small amount of heat, mainly to prevent radiation, but to put in a substantial amount of heat is very difficult in a vertical still. As I previously pointed out, there is substantially nothing to be gained—in an atmospheric pressure still,—because in such a still we do not need to worry about the hot bottom difficulties, we do not care if we have a foot of coke on the bottom. So the thing to do is to use the simple still construction, the efficient method of heating, whenever we can tolerate the coke, as we always can at atmospheric pressure.

Of the reference cited the only ones which showed a structure at all similar are Intschick, Exhibit No. 120, Lennard, Exhibit No. 121; Gaster, Exhibit No. 127; Barbet, Exhibits Nos. 124 and 125; Kornfeld, Exhibit No. 123, and Warden, Exhibit No. 126.

Q. I wish you would tell what is shown in Intschick, Lennard, Gaster and the Barbet of Exhibits 124 and 125?

A. These four exhibits lack both the essential features of operation under pressure, (which as I say is the only thing which creates the utility), and the essential feature of the presence of a considerable body of oil inside the still. Mere side firing is by no means a solution of the difficulties, unless you have a body of liquid in there to keep the still walls cooled, and in these four processes there is merely a film or a spray of oil running down the sides of the tubes, which, as it goes down, tends to become coked. In such processes almost inevitably have some places where there is no liquid flowing down, and in any case except right at the [fol. 942] top, near where the liquid first struck, the liquid would be heated essentially to the temperature of the wall and would exert no cooling action.

So it would be very much as if Adams fired the still above the liquid level, which would have destroyed the utility of his process, because above the liquid the walls could not be cooled effectively. So while this scheme of firing walls cooled only by a spray or film would probably work at atmospheric pressure it would not work under pressure, but the utility that side firing has under pressure is not present at atmospheric pressure. There are two separate distinctions.

In Warden, Exhibit No. 126, we do have a body of oil, but in this case the still is still at atmospheric pressure, thus losing, as I pointed out, the utility of the scheme. Warden does not disclose any cracking at all, but is trying to prevent any carbonization in the tubes whatever. In other words, he discloses that by side-firing a still it is possible to prevent cracking, whereas Adams discloses that by side-firing a still it is possible to get extensive cracking and yet get away from bad carbon troubles.

The Kornfeld reference is the only one which shows any pressure. The pressure being undisclosed in amount, but obviously low, in view of the use of a hand pump to force the oil in. However, Kornfeld is essentially merely a pipe still or tube cracking process, with the tubes working in parallel. He starts out with empty tubes and forces in oil at the bottom, most of which is distilled off. There may be a gradual accummulation of residue rising in the tube, but it cannot rise very high because the bubbles of vapor continually formed would cause serious entrainment in any such narrow tubes. In some cases when he does crack to any considerable extent he indicates that this residue is converted into coke, but in any case, if he does have any liquid level in the tubes at all, as he may part of the time when he is doing but little cracking, he is firing above the liquid level. Most of the time he has no liquid body of oil whatever to keep the tubes cool, and fires both above and below the liquid level, if any instead of just below the liquid level, which is the successful feature of Adams' invention.

Q. Plaintiff's expert contends that the flash or films cracking operations disclosed in these two Barbet patents, Ex- [fol. 943] hibits 124 and 125, might be operated under pressure, because this is described as an improvement upon a previous cracking process—that is, the one you discussed

in Exhibit 136, which he did describe as being operated under pressure. What is the fact on this point?

A. In Exhibits 124 and 125 there is not only no disclosure or even hint of pressure operation, but they could not have been operated under pressure with the structure shown, because the oil feds to them by gravity from a still which is clearly at atmospheric pressure. The unescapable conclusion is that Barbet's improvement over his previous pressure-cracking process, consists very largely in doing away with the pressure. Probably he acquired experience and wisdom as did others who tried to crack such dirty stocks under pressure.

It is also interesting to note that when he did go to the atmospheric pressure film-cracking type of operation he for the first time speaks of making light distillates of the order of gasoline, whereas his bulk pressure distillation processes were devoted to very light cracking to make mainly kerosene.

The Master: The relation of the two Barbet patents, then, is this—that in the Barbet patent in which he employs pressure he does not disclose side-firing; if he discloses side firing in his second patent, he does not disclose pressure?

The Witness: Yes.

Mr. Wiles: And it cannot run under pressure because its gravity——

The Master: Barbet never combined the pressure and side-firing?

The Witness: No. And, in the second place, where he shows side firing, he did not have the body of liquid cool. He simply had a film of liquid running down the walls of the tube, and of course in any operation of that kind, where most of it cokes before it gets to the bottom, you have an entirely inadequate cooling of the tubes.

A. None of the references show side-firing a liquid-filled pressure still, as I have pointed out. The only one which shows any pressure at all, and therefore could even approach Adams' function or utility, is Kornfeld, where the firing is above as well as below the liquid level, and hence would be certain to coke up and give trouble, if any substantial pressure were used; though of course Kornfeld's tubes could be run at higher temperature than a bulk pressure still of any kind.

In addition to this feature, however, the principle of side-firing, as I have already pointed out, cooperates to a very marked extent with another of Adams' features, the use of the agitating and carbon scraping means, and the combination of these two together gives a far more useful result than the mere sum of the two applied separately. In other words, in the Holmes-Manley stills, if we leave out the scraping mechanism, we can only run a matter of half day a day or a day, or possibly two days, before we get hot sides. Furthermore, the carbon scrapers have never been particularly effective, except in a vertical side-fired still. Every place else the accumulation of carbon has been entirely too great to permit them to function successfully.

Q. Which of defendants' processes has this carbon scraping mechanism on the heated surfaces of the liquid filled side fired still, where you draw off your tar and scrapings at the bottom?

A. The Holmes-Manley process, including the Whiting continuous stills.

The practical value of this expedient is that it does permit heat to be put into a large body of oil held at a cracking temperature under pressure, without trouble from overheating of the walls—in other words, maintaining black side, rather than black bottom condition.

I think the exact mechanism of the action of the carbon scrapers has, in the past, been misinterpreted. This is not a thing that we can consider to be entirely proven, as yet, but it is a fact that this scraping mechanism does not positively remove the carbon which has once baked on the walls.

That is shown by the fact that at the close of each ordinary run of fifteen to thirty days on the stills, there is de- [fol. 944] posited on the still walls, generally from a sixteenths of an eighth of an inch of very hard, glass-like coke distributed uniformly over the walls, which can be removed only by sending men in with tools, and a lot of very hard work.

To my mind the function of the carbon scrapers, instead of being actually to remove carbon which is once formed, is to first prevent the carbon deposit from being, or becoming porous, and soaking up oil, and, therefore, forming more carbon, and more rapidly than ordinarily takes place. Second, keeping the carbon deposit uniform in thickness, so as to prevent localized hot spots; and, third, and most im-

portant of all, to scrape off the asphaltic material which tends to collect on the inside of the walls, and get it back into the body of the liquid, and give it a chance to settle out.

In other words, its great virtue is in the agitating action and the removal of the asphaltic matter from the walls, before it is converted into coke. A little of it in time, gets converted, and that cannot be scraped off, but you can very effectively remove this material before it is coked, and get it back into the oil, and then, if you have a vertical still filled with liquid, it will gradually settle down to the bottom. You may have to scrape it off the walls two or three times, before it gets to the bottom, but eventually it will get to the bottom before it is coked, and to my mind that is one of the main reasons why the Holmes-Manley process is able to operate to give so much cracking with so very little coke.

The prior art cited against this feature is Gaster and Lennard, who have two very similar processes, which, from their descriptions you might think were designed to recover the suspended carbon in heavy tars, by spraying this tar onto the highly heated wall of a rather conically shaped vessel, equipped with scrapers.

If it really is merely the evaporation of some relatively light liquid, leaving suspended carbon, it is quite possible that that could be scraped off. That is somewhat similar to the action which takes place in recovering precipitates in certain types of vacuum operations in the chemical industry.

However, the type of materials he is dealing with, seem to me to indicate that he is really talking about a coking operation, and that the carbon which he is talking about securing is the carbon formed by coking very heavy dirty stocks in these tars. Of course, by that means he does get [fol. 945] off a distillate which contains less carbon. If it is a coking operation it is one of the many attempts that have been made both in the petroleum and coal tar industries, to accomplish continuous coking by some such scheme, and every one of them has resulted in failure. We, ourselves, have done considerable work along that line, and I know a number of other refiners have had the idea of getting away from the batch operating in coking, and one of the most frequent expedients employed in that work is that of spraying the oil onto a very hot surface, where it

cokes quickly, and then having some scraping means for removing it from the hot surface.

Of course, we must put in a great deal of heat per unit surface, in order to bring about rapid coking and evaporation, and, therefore, must keep the surface scraped clean, and the experience has always been that in any such case, where the amount of coke formed is quite large, it is impossible to keep scrapers in operating condition. If they are held rigidly, so that they apparently must scrape, in time they simply jamb pnd refuse to work, because the coke builds up so rapidly, and in such large amounts, and if they are kept there by weights and springs, the accumulation of the coke simply pushes back the scrapers further and further, and you get a red hot condition.

Furthermore, they do not have this function of scraping off this asphaltic material into the body of the oil. If they could do that they might do some good, but not having a body of oil, all they can do is to scrape off the asphaltic material onto the scraper arms themselves, and build up a tremendous mass of material thereon, which so overloads the scraper that it can hardly be turned, in a short time.

There is a slight tendency in that direction, even in the Holmes-Manley process, where we do have a body of oil into which to scrape the asphalt.

The simplicity and cheapness and safety of the atmospheric coking operation, has forced out all attempts to replace it with a continuous coking means. Although these scrapers look attractive on paper, they do not work out where you have a large amount of coke to be formed, and they have only been successful in this one case, where the amount of coking is kept at a minimum, and where the amount of firing is very small, and where there is a body of oil tending to keep the surfaces cool, and into which the asphaltic material can be scraped.

And, as I say, Holmes & Manley and Adams have these [fol. 946] features, which make for success, and Gaster and Lennard do not, at all.

Furthermore, Gaster and Lennard do not disclose any pressure, and while plaintiff's expert indicated that they could be operated under pressure, it would require many changes of detail, and since Gaster and Lennard wanted to withdraw only dry carbon from the bottom, inevitably the lower part of the vessel would not be cooled by any liquid

at all, and, therefore, would be red hot under normal operating conditions, coking taking place mostly on the upper part, and it could not stand any substantial pressure, whatever, with safety, in a still even approaching commercial size.

Q. The practical upshot of this art has been that since atmospheric pressure stills will stand the coke, they just let it accumulate and do not attempt to scrape it, at all?

A. To date there has not been found a better method of handling the situation, if we are going to coke, than to run at atmospheric pressure and dig out the coke after it has built up a very thick layer.

Q. On page 499 of the record, beginning at the bottom of the page, Plaintiff's expert indicated that the flowing stream of oil from Pielsticker's pre-heating coil might carry some suspended carbon and might thus anticipate a certain formal description of Adams' carbon removal process. What have you to say on this point?

A. I have not been able to find that this description, beginning with the last line on page 499:

"Heating said oil locally as it passes through said pipe-like heater to a cracking temperature, removing from said pipe-like heater the collecting carbon resulting in said pipe-like heater."

is used by Adams at any point. There may, of course, have been an error in transcription. In any case, if we get away from a mere matter of words, it is evident that Adams' mechanical carbon scrapers for the removal of the adhering carbon cannot be anticipated by a small amount of carbon which might be swept out by a rapidly flowing stream of oil, if such action did take place.

Furthermore, Pielsticker has no disclosure of cracking conditions in his coil, as I pointed out previously—presumably has an outlet temperature around four hundred degrees Fahrenheit—and any local deposition of coke and salt would not be swept out by the rapidly moving stream but would be baked on to the tube and held there.

[fol. 947] Q. You base that last statement on experience with devices of this type?

A. Yes. If carbon is once baked on I do—I feel sure that you cannot sweep it out by any rapidly moving stream of

oil. The main advantage of the rapidly moving stream is in preventing formation of local superheated films, and thus preventing coke formation. If coke once deposits you can double the velocity of the stream without sweeping it out, and this is not surprising when we see the amount of force that has to be expended in the mechanical tube cleaners to break up and get out carbon. It is a very difficult problem to remove baked on carbon.

Q. I think it might be interesting if you would tell the Master just what you use to clean these tubes when the carbon is once baked on?

A. For the smaller tubes we have a special tube cleaner which rotates at high velocities and breaks up and scrapes off the film by the expenditure of a great deal of force. It sounds like a boiler factory when the tubes are being cleaned.

In the stills where the men can get in, they use all kinds of scrapers and other devices for breaking up that very hard adherent film of carbon. Inside of the Holmes-Manley stills they use a hammer very largely to crack and chip off the adherent film.

Q. As a matter of fact, do any of the prior art pressure cracking processes show any means whatever for removing the carbon from fired surfaces into an unfired zone while the process is operating?

A. No, not so far as I have been able to find.

Q. Now, the next feature of Adams which you enumerated has to do with the use in a system of cracking by pressure distillation of a plurality of vertical stills operated in series with a common vapor line. State which of the defendants' practical processes employ this feature?

A. This feature is employed only in the Holmes-Manley process, including the continuous stills at Whiting.

Q. What references does the plaintiff's expert cite against this feature?

A. Plaintiff's expert cited Bacon, Exhibit 122, Intschik, Exhibit 120; Barbet, Exhibit 124 and Kornfeld Exhibit 123.

It is mainly emphasized in Patent No. 1,462,677, Plaintiff's Exhibit 198.

I find on page 2100 of the book of exhibits, line 111 of this Adams patent, this statement:

"This unit battery feature and the freely-connected converters, one with another, in its broadest aspects, was first adopted by me and employed as described in my co-Pending application, serial No. 530,852, filed December 1, 1909, which matured on October 28, 1919 into patent No. 1,320,354, and it is claimed in Patent No. 1,433,519, which is a division of said patent and was granted on October 24, 1922."

This statement which I find is correct in point of fact, indicates that this idea of Adams ante-dates the Bacon reference, Exhibit 122, which would therefore not need to be further considered.

As I pointed out, in any batch pressure distillation process, the character of the tar progressively changes as time goes on, and it frequently necessitates a change in the operating conditions to best handle that tar without getting too much coke, and yet continue to crack it at a reasonable rate.

In a continuous process if you have only a single still, you have in that still at a given time a mixture of feed stock which has been in for a short time, and feed stock which has been in for a long time, and if you have but a single still without partitions you have to hit an average of conditions which are best suited for that mixture rather than car-ying the condition as might be desired to handle the tar best in its different stages; but if you pass the tar [fol. 948] continuously through a series of stills in which it gradually changes, you can then modulate the temperature conditions and the firing in such a way as to maintain the optimum for conditions for each stage.

In addition to that, the adoption of the sectional feature, or the series of stills, has the advantage which I mentioned yesterday, that it prevents the fresh feed from getting mixed up and going out the outlet too quickly; it makes it take a reasonably orderly course in passing through the stills and cracks substantially all the material going in to, roughly, the same extent.

Q. Now, having shown that this ante-dates Bacon, I want you to discuss the applicability to this feature of the other three references, Intschik, Barbet Exhibit 124, and Kornfeld, which are cited against it.

A. In the first place, none of these three are bulk pressure distillation processes in any sense. Even more striking than that distinction, however, in many ways, is the fact that while they do have several units they are independent units operating in parallel; and, therefore, they lose entirely the benefit of this feature of Adams of putting the oil through a series of converters and being able to control conditions in each one as the tar changes.

They have none of the advantages which I have mentioned of series battery operation in preventing bypassing and in permitting regulating.

The Master: You say none of them are in series?

The Witness: None of them are in series.

A. (continued). The only one which has any suggestion of any kind of series operation is Intschik, who suggests that he may, if desired, take the vapors from one of these stills and pass them on through another one for further cracking; but, of course, that is exactly the opposite of what Adams wants to accomplish. He wants to take his vapors out as fast as they are formed through this common vapor line and send the liquid, the uncracked material through a series of stills in order to bring about the desired change.

Q. Now, the next discovery of Adams which you enumerated has to do with the re-running of the heavy distillates [fol. 949] produced in a gasoline-making bulk pressure distillation process. State first which of the defendants' processes use this?

A. In one form or another, limiting it to a bulk pressure distillation process of course, the Holmes-Manley and the Burton processes are the ones which employ this feature regularly and in such a way as to very greatly increase the yield of gasoline.

As I have already pointed out, it was old and well-known in the tube-cracking art to re-run the heavy portion of the distillates, and in that art it created no problems. It was simply like so much fresh stock, because the boiling point of the stock has practically no effect on a tube cracking process, and the products all go along together, vaporizing at some intermediate point in the tube, as a rule.

As I also pointed out, however, the handling and re-cracking of the heavy distillates in a bulk pressure distillation process creates a number of very real and important problems in the proper correlation of temperature and pressure and in the ability to get a rapid racking rate in spite of handling light stocks which tend to boil away at too low a temperature; such processes are the only ones that are possibly applicable as a reference against this use of Adams.

The three bulk pressure distillation processes which were cited against this feature were Pielsticker, Dewar & Redwood and Barbet, Exhibits 136 and 141, I have already discussed in detail, with the aid of the large chart, the alleged recracking of distillates from the Barbet pressure still, and pointed out that according to Barbet's disclosure there was no possibility of any appreciable amount of material which ever left the still getting back into it.

The case of Pielsticker and Dewar and Redwood I have already discussed in considerable detail and pointed out that the heavy distillate which they re-cracked was very small in amount and consisted primarily of the heavy ends.

Furthermore, they were making kerosene, so the type of material which they wanted to put back was only material in the gas oil range and higher, which is a very different problem from that of re-cracking material in the low-boiling kerosene range. The material which Adams discloses the possibility of re-cracking and does re-crack, is the desired final product of these other kerosene making processes, and is much more difficult to handle in any kind of a bulk pressure distillation process, than much smaller quantities of the mere heavy ends and heavier than kerosene.

The Master: Was there a process adapted to handling the heavy ends from a gasoline-making process?

The Witness: Pielsticker certainly was not in any sense adapted. Dewar and Redwood make no disclosure of the extent of pressure and no disclosure of the correlation of temperature and pressure which might be required to handle any such product; and, as I pointed out, the most important thing in handling these light distillates is the difficulty of re-cracking them in a pressure distillation process,

on account of the fact that they boil so low. And in the Dewar and Redwood type of process I do not think it would be possible, without a very much more definite disclosure, either to return the products to the still or to recrack them when they got back, although I would not say, with all we knew today, we could not make the changes which might be necessary to accomplish that.

[fol. 950] The feature which is disclosed by Ellis in his patent No. 1,415,232, plaintiff's exhibit 192 is essentially the disclosure of the fact that by taking the products of a tube-cracking process and passing them into a large drum where they are held at a temperature equal to or slightly below the coil outlet, in either a lagged or a very lightly fired chamber, and still under pressure, it is possible to produce further substantial change—in the products which have come out of the cracking tube proper.

This feature is, of course, one of vital importance in the tube and tank type of process. It is the feature which I mentioned as having been added to the prior tube-cracking art and made what were unsatisfactory non-commercial processes highly commercial and efficient processes. It is, therefore, a vital element, as I indicated, in the Cross and tube and tank type of process, and also the same feature is embodied, in its broad sense, in the Holmes-Manley method of operation, because while they fired their drums lightly they did not raise the temperature. The temperature in the stills is lower than the temperature at the coil outlet, as Ellis provides. Also, you will note that Ellis specifically illustrates in his patent and shows that the drum is surrounded by the low temperature flue gases after they have passed through the coils, giving a somewhat similar very mild heating.

It is a little difficult to be certain of the Ellis phase from the Ellis patent and file wrapper, and of course, being a modified tube-cracking process, as I pointed out, it really does not make much difference, except to some extent in the capacity of the apparatus, whether it is mainly vapor phase or liquid phase operation. We undoubtedly start out with mainly liquid and undoubtedly finish up with a large proportion of vapor. If the charging stock happened to be quite light, such as kerosene, and the pressure fairly low, it would be mainly in the vapor phase at the end, whereas, if the charging stock were heavy or pressure were high, a

larger proportion would be in the liquid phase. However, even with very light stocks initially fed into the system, and comparatively low pressures; which would tend most toward the vapor phase, there would of course be liquid at the coil inlet and there would inevitably be synthesized a certain amount of heavy material which would fill up the lower part of the drum below the liquid outlet, which you will note is about the middle of the drum, so that there will be inevitably in Ellis, shortly after the process gets under [fol. 951] way, a substantial pool of liquid held in the reaction chamber.

As I indicated, however, in practically all of these tube-cracking processes, both vapor and liquid are present and their relative proportions are not a matter of great importance one way or the other.

Plaintiff's expert stated in substance, based on the file wrapper, that Ellis did not contemplate cracking in the drum or reaction chamber because he stated that the drum had the function of stabilizing the product. As a matter of fact, the installation of the large insulated or lightly fired drum at end of the cracking tube had precisely the ultimate result which Ellis claimed; that is, of making possible the production of a more stable gasoline than would otherwise be the case, although we now think that that is mainly due to the further cracking which takes place in the drum, although in part to some stabilizing reactions as well. It would appear from the original application and in the earlier part of the file wrapper of the Ellis application, that Ellis believed that the reactions which took place in the drum were qualitatively different from those which were taking place in the tube, although we now believe that he was in part, mistaken in that, the directions which he gives lead inevitably to the result which he claims, that is, of getting a more stable product and there is no mistake, in any case, in the directions, regardless of his being partially mistaken on the theory. What we today believe really accounted for Ellis' results in large part, is the fact that by putting a drum on to the end of a cracking coil, we automatically lengthen the time that the oil is in the system, and therefore we can lower the temperature, compare on the one hand a cracking tube, handling a certain amount of certain stock, to give a certain yield of gasoline, and the same tube, plus a tank running under such conditions

as to give the same yield of gasoline, we will find that the second process will run at a lower temperature, will give a much smaller content of unstable, highly unsaturated hydrocarbons, and smaller gas loss and smaller heat consumption.

Ellis attributed this difference in the products obtained from the two kinds of processes in main to a stabilizing action taking place in the drum. Even today we do not know just what reactions do take place in that drum. We do [fol. 952] know that there is at those high temperatures and pressures a certain tendency to polymerize and eliminate gum-forming constituents, but as I indicated we believe that the most important changes which take place in there are the further cracking which takes places at the high temperature which prevails.

The Master: Assuming the stock to be the same, would there be, as comparing with the Holmes-Manley still, the same function and substantial identity of result?

The Witness: The two processes being different in that the Holmes-Manley process does distill from the still makes the heat requirements, the amount of gas loss and the amount of coke deposited somewhat different between the operation of the tube and tank and that of the Holmes-Manley, but the essential feature of bringing the oil to a cracking temperature in a coil, but doing a substantial amount of cracking in an unfired or lightly fired drum or still, are common to both processes.

As I indicated Ellis was apparently partialy mistaken as to his theory of what took place, but there was no mistake as to the directions which he gave for bringing about a desired and very valuable result.

The broad result of the more stable product does occur. This Ellis procedure can be carried out perfectly well, in either the liquid or the vapor phase, with beneficial results, and in general quite similar results.

Plaintiff's expert, as I recall it, agreed that at a given temperature the nature of the cracking reactions was quite similar in the two phases, although he stated that due to greater danger of local overheating in a vapor phase process he would expect a somewhat greater amount of unstable compounds, which is quite possibly true. Of course, the Ellis type of process is very well suited to avoid danger of undue local heating, most of the heat being put in in a

rapidly moving stream, and the drum being but very lightly heated.

A good many experiments have been carried out on cracking the same kind of stock at about the same temperature for the same length of time in the two phases, and [fol. 953] there are in general small but unimportant differences between the results of the two methods of operation, providing the other conditions are kept constant. Of course, ordinarily a vapor phase operation is operated at a higher temperature than a liquid phase operation, which would automatically make a difference in the result. That difference is due to the difference in temperature and not to the difference in phase.

The references cited against this principle of Ellis's were the four prior art tube-cracking processes which had separating drums—that is, Kittle, Benton, Schuchow and Gavrilow, and Turner, together with Pielsticker. Regarding the first four of these, the tube-cracking processes which were equipped with separating drums to separate the tar from the vapors at the outlet of the coil, the position of plaintiff's expert seems to be that Ellis' drum was merely an enlargement of these separating devices in the prior art patents.

I have already discussed that point rather briefly, but I think we need to go into it in detail to clear up the whole situation. In the first place, as I pointed out, Ellis' drum does not have the separting function of the prior art drums. Ellis still has to have his separating drum beyond his digesting drum; Ellis has inserted a new element between the cracking coil and the separating drum or separating chamber of the prior art process.

Ellis accomplishes his separation in the base of his tower, just as the tube and tank process, and the Cross process, accomplish their separation at that point. Furthermore, Ellis' drum does have the very important function which the prior art drums lack. To be effective for carrying out Ellis' idea of further substantial change in the oil without a rise in temperature and without much fall in temperature —in other words, maintaining it in the active cracking range—the drums must possess three characteristics: First, large size, because we want a long time to carry out these other operations, so that we can do a modified tube cracking at low temperatures. Second, accurate control of the tem-

perature within definite limits. Ellis makes very clear that you must allow the temperature to drop but slightly, and that is the prime essential for the success of this type of operation. If it drops too low you do not get the desired [fol. 954] further cracking, and to make it go higher you would have to fire it so hard as to lose the advantage of coke deposition in an unfired or only very lightly fired vessel.

Third, a liquid outlet high enough to hold a substantial body of liquid in the drum, the point in that being that it is particularly the heavier constituents you want to be sure to crack, and if you have your liquid outlet near the bottom of the drum, that means the liquid, as soon as it goes in the drum, will go out without being subjected to the further cracking to which we wish to subject it.

With regard to these features, examining the four prior art references, of course Ellis has all three of these essential features, whereas, taking the manner of the liquid outlet, all four of the prior art separating drums had their liquid outlet at or near the bottom, which is just what you want for a separating drum to get the tar out as soon as possible, but just what you do not want for a reaction vessel.

With regard to the size and temperature, there is no specific mention made in any of these four patents regarding either of these two vital features. A person skilled in the art, taking the disclosure, would naturally build the separating drums of such size as to accomplish the separation which was their disclosed function and their only disclosed function, and as plaintiff's expert has said—and as is true around the refinery—separation is ordinarily a matter of seconds. It takes a very, very short time in any kind of properly designed separating drum to mechanically separate the entrained tar from the vapors, whereas a matter of an hour or more is ordinarily required for substantial further chemical change, although of course considerable change can be made in a half hour, say, if the temperatures are high enough. But a matter of seconds is not adequate to bring about the desired chemical change, though it is quite adequate for separation.

Furthermore, even if we had the right size, there is no disclosure whatever as to temperature. But even if we

should give the prior art the benefit of Ellis' disclosure, that they wanted to maintain the oil in this body at a temperature slightly below that of the coil outlet but still in an active cracking range, there is not one of them shown or constructed which would be capable of maintaining that temperature for any considerable length of time. Thus, in Kittle, both the drum and the coil leading to the drum [fol. 955] were immersed in a lead bath, and he says that the temperature of the oil going through the coil is 360 degrees below that of the lead bath; therefore, there must be further very substantial input of heat into the drum, and the drum temperature would necessarily be higher than the coil outlet temperature.

In Turner the vapor merely passes through the upper part of some long unlagged pipes, and the liquid settles down to the bottom, and particularly in the bottom where there is no repeated contact with hot vapor, the pipes merely acting as separating chambers, the body of liquid would inevitably fail to quite a low temperature, certainly outside the cracking range.

In Schuchow and Gavrilow not only is there this cooling action due to radiation, but this is, as I pointed out, accentuated by the introduction of cold feed into the chamber.

Plaintiff's expert stated on page 841 of the record that if it were operated to make gasoline, of which, of course, there is no disclosure in the patent, a suitable temperature to be maintained in that drum would be 450 to 500 degrees, which is of course far below a satisfactory cracking temperature.

As to Benton, if you will refer to that drawing in Exhibit No. 150, Benton's separating chamber is fired at the bottom and exposed to direct radiation without any lagging on the sides and top. Furthermore, Benton reduces his pressure just as it comes in, so he has a large amount of cooling due to and the sudden flash vaporization of the material which comes into the drum.

I presume it is conceivable that by very hard firing at the bottom of this vessel it might be possible for a short time to maintain the temperature of the liquid in the bottom of the drum at some desired point if you knew what you wanted to maintain, but any such hard firing as would be required to overcome the large amount of radiation and

the cooling due to self-evaporation, directed against the heavy tar that would be left in the bottom of this vessel at atmospheric pressure, would coke it up in a very short time, in which case no matter how hard you fired you would not get enough heat to maintain the operating temperatures which you desire to carry out the Ellis invention.

As to the last reference, that of Pielsticker, I have already pointed out in other connections that Pielsticker's disclosure indicates hard firing of his drum and a rise of temperature in his drum, which defeats the important result of Ellis, of depositing carbon in the unfired zone. That would also be true of Benton. If we did fire Benton as hard as would be necessary to keep it anywhere near the right temperature we would be depositing the coke in a strongly fired vessel, which is what Ellis wished to avoid. Furthermore, of course, Benton's drum is not under pressure, so the digesting action would be quite minor.

[fol. 956] The Master: Going back to the drawing of the Ellis patent—please direct my attention to the feed and the flow of the materials through Ellis.

The Witness: The flow comes in through the pump, 16, which is below the center of the drawing, in the upper part of the coils, which are arranged into a series of four parallel coils, and then passes down through the coils in the furnace, toward the fire, the fire getting hotter and hotter. The liquid then comes up in the four parallel pipes, and then discharges into the drum, which is surrounded by the relatively cool furnace-gases, which have passed the coils.

The materials leave this vessel through the pipe, 9, the outlet to which is just above the inlet pipe, and discharge into the bottom of the tower, where, first, the tar is separated, and then, at higher points in the tower are intermediate cuts of material are taken off.

The Master: Now, where is the opportunity for loss of temperature in the drum, A?

The Witness: The question of whether you would get a loss, or gain of temperature, there, is a thing that would be difficult to tell, without some definite statement, which—

The Master: All the heat of your firing is going up through the tubes and around the drum, 8.

The Witness: Yes, sir, but, you see, the gases, by the time they reach that drum, have gone past the tubes which contain the cold oil, and may be actually cooler than the oil

within the drum—or, at practically the same temperature as the drum.

Now, in the drum there are necessarily certain factors which absorb heat. We believe there is a definite heat absorption to bring about the cracking reaction, and there is also in the drum a certain amount of vaporization taking place. That is, material leaving the liquid phase and going into the vapor phase, and absorbing a certain amount of heat, even though there is no separation, because both go out the same pipe.

The Master: Is there condensation under pressure, here?

The Witness: I am not certain on that point. I will look into that.

The Master: Where is the pressure released, starting in with the pump pressure?

The Witness: It looks to me as though the pressure was released in the three valves at the right hand side of the tower, and that the tower is under pressure—that is, the valves 11, 11-A and 11-B—but I will have to read, to make sure on that point.

The patent makes no definite statement as to just where the pressure is controlled, although it does state definitely, on page 2046, line 12: "The apparatus may be run at a slight pressure, or at considerable pressure, as several atmospheres may be employed, if desired." If operating in that way, with the apparatus shown, he would undoubtedly govern his pressure mainly by means of the valve, 14, for gas release, and would also have to keep his valves 11, 11-A, and 11-B, throttled sufficiently to prevent loss of pressure through them.

The Master: Well, assuming that he can obtain pressure, if desired, through this arrangement—that is, Ellis—what is the distinction between this and Cross?

The Witness: There is not much distinction between this and Cross—in fact, as I have stated, this invention is used in the Cross process, and, of course, Ellis is earlier in his date than Cross.

Plaintiff's expert stated in his direct examination that Kittle employed in that part of his process that he cited—the one that you have just been discussing—pressures of 6.45 kilograms per square centimeter, which corresponds to about ninety-five pounds per square inch.

It is very clear, from a reading of the patent, that the pressure mentioned is the pressure in the oil reservoir, [fol. 957] or the pressure at which the oil is supplied—that is, the oil reservoir, A, fed by the pump, L—referring to the figure in the patent.

There are several matters with regard to this figure, and you may wish to refer to it, in "Exhibit No. 131," page 1642—you will find it there.

I am pointing to the pump, which forces the oil into the feed chamber, and you will note that between the feed chamber, which is kept at this pressure, and the cracking coil, there is a valve used to control the rate of feed.

From the discussion of the operation it seems apparent that practically all of this pressure is lost by friction through the valve and through the coil, because it states that the pressure in the separating chamber, F, is maintained by a constant head of oil—I-I'—, in the chamber, K'—that is stated as the means of creating pressure therein, it being desired to supply gas for these gas engines, and for such other purposes as might be desired.

We can get an idea of the magnitude of this pressure, from the fact that the tube is specified in the patent as being six and one-tenth meters long—and since this is arranged in eight coils, that figures out that the coil as a whole is about ten inches in diameter—and, based on that, the head of liquid would be a matter of something like eight inches. In other words, it would give the few ounces of pressure at which gas is characteristically supplied.

You would have to use a head several feet in height—I think about eight feet—to get even as low a pressure as three pounds.

Q. So your position is that the feed pressure is dissipated in the passage through the coil?

A. And through the regulation valve, A'.

Q. Now, that is about a hundred pounds pressure drop, in going through the valve and the coil. How much pressure drop did you tell us, yesterday, that you had in the Holmes-Manley process?

A. From forty to 100 pounds.

Q. That is, the pressure at the inlet end of the coil is 40 to a hundred pounds higher than the pressure where the oil enters the reaction chamber?

A. Yes. Such drops in pressure are common in processes [fol. 958] where oil is forced through a cracking coil, at fairly high velocity.

Q. So that the dissipation of this pressure by the passage through the coil, would not be at all surprising?

A. It would not be surprising, even if there was no valve, but having a valve, which is used to regulate the flow, you generally want to have considerable excess pressure back of the valve. As the coil tended to clog up you could open the valve wider and wider, and have extra pressure to force the oil through the tube.

Q. Now, when on cross-examination I called to the attention of plaintiff's expert, the oil head that the patent says regulates the pressure, and he then tried to maintain that the entire vessel, K, was under pressure, and that the effect of the head, I-I', was to add a few ounces to the super-atmospheric pressure in the whole tank, K—what is your position on that?

A. Not only is there absolutely no disclosure of pressure in the tank, K, but it is clearly shown to be a vessel with an open top, directly open to the atmosphere.

Q. Now, plaintiff's expert maintains that the line across the top of this, indicated that it was closed—is that in accordance with the rules of drawing that are commonly employed?

A. No, it is not. When any vessel is shown in cross section, as this vessel, K, is, and, indeed, as all the vessels in the drawings are shown, a single line simply indicates the top of the far edge of the vessel.

You will note, for example, the clear distinction between the pressure vessel N-4, which is shown as having a cover, and the open condenser, N-7, which is shown with an open top. The same distinction is made between the vessels, P-1 and P-4, in Fig. 3, and between S and the last vessel in Fig. 4.

There could be no question but what this is an open top vessel, and doesn't have any means of maintaining anything but atmospheric pressure therein.

Q. Then, the pressure at the end of the cracking zone is just what the patent states—that maintained by the oil head I-I'—is it not?

A. Yes, sir.

The Master: And the mention of one would under the ordinary rules of interpretation, exclude all others?

Mr. Wiles: Yes, sir.

Q. Now, in his cross-examination, on page 751, plaintiff's [fol. 959] expert insisted that there would normally be a liquid level in the little drum of Kittle, as shown in the drawing, in Fig. 1—is that true, or is there an error in the drawing?

A. There is either an error in the drawing, or an error in the statement of the patent, because, if this head, I-I, is to be effective in maintaining any pressure, whatever, it must contain vapor on the inside, and liquid on the outside.

If there were liquid both inside and outside, it is obvious that the tube could be cut off up at the top of the liquid, and have no effect on the pressure. It is only by balancing a head of vapor against a head of liquid that you can maintain even those few ounces of pressure.

Now, if that pipe is full of vapor, of course there is some liquid coming down—that would mean that the liquid must be flowing down the sides of the pipe.

But if that is true the liquid cannot stand full above the outlet in the small drum, F. It must be just dribbling down around the sides of the tube, and allowing the tube to remain filled with vapor.

So that we are forced to the conclusion either that there is no liquid, at all, in the drum, F, or that the only means of pressure maintenance in F, is not existent, and there is no substantial pressure, or even a few ounces in the drum F.

Q. It just depends, doesn't it, upon the proposition of liquid seeking its own level?

A. Yes, sir.

Q. And if there was liquid in that drum it would just run out?

A. Yes, sir.

Q. Mr. Wilson, early in your testimony you produced a copy of the Humphreys, Rogers & Paulus patent, which you said outlined the principles which were employed in high pressure cracking. In which of the defendants' processes are the principles outlined in that patent employed?

A. This patent outlined particularly the underlying principles of high temperature and high pressure bulk pressure distillation, rather than cracking as a whole, which principles are employed in the Whiting high pressure still, in the Whiting continuous stills and in the Holmes-Manley process, as ordinarily carried out.

Q. You also stated, in the course of your testimony, that the modern improvements have enabled us to handle dirtier and dirtier stocks. Please tell us just how far that has gone, now.

A. With regard to the Burton process, I believe I have already indicated just how far we can go.

In the Holmes-Manley process it is, as I believe I have indicated, possible to run for a very short time—possibly as long as a day—on reduced crude. But the fact that the stills are fired, and that the bulk of the cracking takes place there, even with the light fire, and the scrapers, and all the other improvements, makes it impossible to make a run which [fol. 960] pays, economically, on reduced crude. Although, as I say, it undoubtedly is possible to run for a few hours or a day.

The Master: Does that few hours come at the beginning, or at the end of the run?

The Witness: I mean, that if you start up on reduced crude, you will be able to crack for a few hours, instead of running for fifteen or thirty days, as they can on clean stock.

In the tube and tank process, on account of trying to do a considerable amount of the cracking in the coil, they find it necessary to have almost as clean stocks as in the Burton process, if they are to do any considerable cracking, although they can handle reduced crude to do very, very light cracking, the function of which is mainly to reduce the viscosity of the oil.

In the Cross process, on account of the fact that they minimize the cracking in the tubes, and heat only to incipient cracking, so to speak, and carry out the rest of the cracking in the drum, means that they can run for some little time on crude without serious danger, the main difficulty which arises is that the reaction chamber fills up very rapidly with a large amount of coke, and, therefore cuts down the length of the run, but they have gotten away from most of the fire hazard in this type of process, because of the fact that they do separate the zone of heating and the zone of cracking more completely than in any other process.

But even then I do not consider, really, that it gives the best results under any but quite exceptional circumstances, to operate in that way, but it is technically possible for this process, to make runs of reasonable length, even with crude.

The Master: You mean the best results as to products, or the best results economically?

The Witness: The best results economically, as to the total profit in a properly designed plant. But in certain cases it might very well work out best, at times, to run the Cross process on reduced crude.

Q. In his testimony plaintiff's expert stated that the idea of auto-tar distillation—that is, distillation of the tar by the release of pressure when it left a pressure still, as shown in the Clark patent No. 1,410,797—was in his judgment anticipated by the Barbet disclosure. What is your opinion on this point?

A. This Barbet disclosure was not cited against this feature in the Patent Office and was not known to me until Dr. McKee cited it; but I am bound to say that it does seem to contain substantially the same idea, where Barbet shoots his tar from his cracking process into a bubble tower and speaks of the automatic distillation which takes place on the release of pressure.

[fol. 961] Q. What is the nature of the disclosure of Burton product Patent No. 1,167,884, which I shall for convenience refer to as the Burton cycle stock or twine oil patent?

A. In this patent Burton describes the distillate which he obtains from his pressure tar produced in his cracking process starting with gas oil. As I pointed out before, Burton was the first one to use gas oil in a bulk pressure distillation process of this general type, and was therefore the first to hold the gas oil for this prolonged period under cracking conditions, with the formation of a very large percentage of saturated heavier hydrocarbons of an unusual type, and, furthermore, he was the first to take this pressure tar and distill it and separate and recover the distillate.

It is therefore almost necessarily true that in a narrow sense Burton had a new product—that is, a product containing either new compounds or compounds in an entirely new proportion. It might or might not be true, however, that this product which he obtained by first the cracking operation and, second, by recovering the distillate from the dirty pressure tar, would be substantially different in its physical properties from other petroleum products. But Burton found upon examination that while this oil, like any ordinary series of oils, increased in viscosity as it increased in

specific gravity, for any given specific gravity the viscosity was abnormally low.

I want to emphasize that Burton is not patenting or not disclosing an oil which is remarkable in having a low viscosity, by itself, but in having a viscosity which is very low in view of the gravity. In other words, it has a unique combination of physical properties. It is a good deal as if we should consider a series of children in one family. We might find that in the family we would have children ranging in age from two to ten years, and in height from three to five feet—a gradual progression—and we have a series of ages represented and a corresponding series of sizes represented; whereas, another family might have a family of dwarfs, where they have a similar variation in size and a similar variation in age, and the taller dwarfs would be taller than the shorter members of the normal family. But for their age the dwarfs would be short in height. In other words, the family of dwarfs would in this respect represent an unique combination between age and height, the same as Burton's oils represented a unique combination between viscosity and gravity. Of course, this unique combination is only a reflection of the unique types of chemical compounds which are produced by this process, but it does afford a simple and easy method of distinguishing between Burton's new product and any other petroleum product that has been found, I believe, either before or since—except, of course, that other cracking process developed since his time and operating on gas oil naturally give a similar product if their pressure tar is distilled. This tar happens to have particular value for use as a twine oil, though its main use is as cycle stock for the further cracking.

[fol. 962] If you take any series of oils from a given crude or from a given product, made by cracking, we always find that as we raise the still temperature the products grow increasingly heavy in their gravity and increasingly thick or viscous. I am assuming, of course that cracking is not taking place during this distillation. It is possible, if you bring about cracking, to get some very light oil off at a high temperature, but any series of oils separated by simple distillation, as plaintiff's expert has also agreed, show a gradual tendency to increase in gravity and in viscosity with the increasing boiling point of the materials present.

The Master: Is that by reason of the lighter parts going off?

The Witness: Well, it so happens that in a series of this kind, such as we get in petroleum or in pressure tar or any general series where we have a large number of compounds of roughly similar type but varying in boiling point, that the lowest boiling ones are at once the lightest and the least viscous, and the higher boiling ones are at once the heaviest and the most viscous. You might, of course, make up an artificial mixture, in which that might not be true. Take a mixture of benzol and two or three other compounds, and that would not necessarily be true. But in dealing with petroleum products or products of cracking petroleum products, I believe that is an obsolutely general rule. In practically all composite mixtures of similar types of hydrocarbons, that general rule will be found to hold, and is always assumed and understood by those familiar with the art.

Of course, if we compare different series like Burton's twine oil, we find we do have an unique relation. That is to say, the relation between gravity and viscosity for that series of oils will not be the same as for the series of oils made, say, from Pennsylvania crude or another series of oils made from a Gulf Coast crude, and so on. But for any given crude, that is the general rule.

Q. And the Burton oil, you say, is unique in this relationship between the gravity and viscosity of the oils?

A. Yes.

Q. How did Burton measure the viscosity and gravity of his new series of oils?

A. Burton measured the gravity with the ordinary Baume hydrometers in wide use in the industry. The Baume scale, which is used in the oil industry, has always in the past been called Baume, although it differs a little from the standard Baume scale used in certain other industries, and for that reason, in order not to make a change in the scale used, and to avoid confusion, the Baume scale, commonly used for many, many years in the oil industry, has recently been renamed the A. P. I. Scale, after the American Petroleum Institute, which adopted it, and we now have conversion tables between specific gravity and the A. P. I. gravity, which is the oil industry's Baume scale.

This Baume gravity scale has the advantage that it gives smaller numbers for the operators to remember, as compared with specific gravity, where you have to remember three figures at least to get a sharp cut, while in the Baume

scale it is so spread out that you only ne-d two figures. It has the disadvantage, however, that the higher figures correspond to the lighter gravities and the lower figures to the heavier gravities, so it tends to be a li-tle confusing. For that reason I always try to speak of light and heavy gravity rather than high and low gravity, because the terms high and low are somewhat deceptive, depending on whether you are considering the Baume scale or the specific gravity scale, where the larger numbers correspond to the heavier oils.

The instrument he used for measuring viscosities was also [fol. 963] the one then and now by far the most commonly used in the petroleum industry for measuring the viscosity of materials in the lubricating range and slightly below, namely, the Saybolt Universal Viscosimeter. This is a very well standardized instrument, undoubtedly somewhat better standardized today than in Burton's day, but they have always been very carefully made to give reliable comparative results. These were invented I believe by Mr. Saybolt of the Standard Oil Company something like twenty years ago.

It measures the length of time required for sixty cubic centimeters of the oil to flow out through a capillary tube of certain dimensions at a given temperature, and the viscosity is recorded in seconds. That is, if we say the viscosity of a certain oil is sixty seconds, that means it takes sixty seconds for sixty cubic centimeters to flow out through that particular capillary. If we say it is 200 seconds viscosity it means that sixty cubic centimeters of the oil take two hundred seconds to flow out through this capillary under properly specified conditions. The higher numbers in this case indicate that the oil is thicker.

The ordinary household lubricants, very light oils for sewing machines and light tools, generally run in the neighborhood of one hundred seconds viscosity, at 100° F., while medium motor oils generally run in the neighborhood of three hundred seconds viscosity.

In Europe the instruments generally employed for gravity were similar in their general construction, but the scales used were the specific gravity scales. That is, they measured on their scales the relative weight of a given volume of oil as compared with that of an equal volume of water. So that a specific gravity of nine-tenths, for example, means that a given volume of oil weighs just nine-tenths as much as

the same volume of water, under certain specified temperature conditions.

[fol. 964] The viscosity instruments used, on the continent at least, were mainly the Engler viscosimeter, which is an instrument working on a somewhat similar principle but is less well standardized than the Saybolt universal. It reports the figures as a rule in numbers which represent the ration between the time requierd for a given quantity of oil to flow through a given capillary and the time required for an equal quantity of water to flow through the same capillary. As I said, the Engler instruments, at least those received in this country, have apparently not been as well standardized. That is, they give appreciable deviations between one another, although for most practical purposes they are good enough, and they are used to some extent in this country, particularly on the heavier road oils.

Q. So that an Engler viscosity, say of 4, means that it took four times as long for the oil to flow through the hole as for that much water?

A. Yes.

The Master: Necessitating there a multiplication or computation:

The Witness: Yes.

The Master: While the Saybolt is a positive declaration of time.

The Witness: The reason for the Engler scheme was that in a way the water is used to calibrate the instrument, and if the instrument is not quite standard, that is to some extent taken into account. Sometimes Engler results are reported in seconds as well.

Conversion tables are now available by which it is possible, if we know the viscosity on one scale, to calculate with the aid of that table the viscosity on another scale, and similarly for gravities. Those are tables that are well known, and tables that are now furnished by the Bureau of Standards for both these properties, which are highly accurate. Some of the earlier tables, based on less careful work and a smaller number of instruments, are not so accurate.

Plaintiff's expert and I agree that the Bureau of Standards tables are the most accurate at present available.

[fol. 965] Q. Plaintiff's expert made the point that the Bureau of Standards conversion tables were not available before 1920, also that the different viscosimeters or viscometers might vary somewhat from the standard. Will you please state what bearing these points have on the conversion of prior art Engler results to Saybolt results.

A. While both these statements of plaintiff's expert are correct, I do not see that they have any real bearing on the problem of converting Engler results to Saybolt results. What we are interested in is not what somebody thought Engler may have had in 1915, but in what the properties of the oil which Engler had actually were. In other words, what results Engler's oils would have given if put into a Saybolt viscometer in order to compare the results with Burton, and for that purpose we certainly wish to use the most accurate conversion tables which are available. In other words, we wish to arrive at the actual properties of that oil, not what someone may have erroneously thought they were because he did not happen to have available a satisfactory conversion chart.

As to the fact of the existence of non-standard instruments, it seems to me that all we can do in any such case is to assume that the prior art references were using a standard instrument and were using it in a standard way, because we must do that if the prior art is to mean anything at all. If they did not use a standard instrument or did not use it in a standard way, it is just as if they had given no description whatever of their properties.

Take a patent like that of Riebeck, for example. When Riebeck said he had an oil of .93 specific gravity and said that it had an Engler viscosity of seven, either that statement means nothing at all or it means that he used all due care and the best instruments that were available and ran them under the prescribed conditions and obtained these results. An- that in turn means that if he took these same oils and ran them in the Saybolt instrument and with the Baume scale he would obtain the figures of 20.65 Baume and 239 seconds Saybolt, which are the figures given by

the Bureau of Standards conversion tables. To assume anything else simply makes the prior art meaningless.

Q. How did Burton in his patent, which is Exhibit No. 171, show the unique relationship which you state exists between the viscosity and the gravity of his new oils?

A. He gave in his table on page 2 of his patent, page 1916 of the book of exhibits, a series of gravities and of viscosities for different types of oils. First his own new product, the twine oil or cycle stock, where he had a series of gravities varying from 28.9 to 10.8 and the corresponding viscosities of each cut, varying from 36 to 198. He also gave a typical series of oils from a California crude petroleum covering a similar range of viscosities and a series of oils from a Kansas-Oklahoma crude petroleum, or what we generally call a mid-continent crude petroleum.

To illustrate the point which Burton was trying to make by this table, refer first to the last table for mid-continent oils and we see that there in the middle of the series is an oil of 26 gravity, which has 165 seconds viscosity. If we refer to the next table above for California oils we find there an oil of 167 viscosity, which is the nearest viscosity [fol. 966] to the one in the lower table and the corresponding gravity is 21.8, distinctly lower than the 26 gravity for the oil in the other series. In other words, these two oils are characteristically different in their gravity-viscosity relationships, and indeed that is one of the common ways of telling what type of crude an oil came from, by finding these differences in gravity for a given viscosity. When, however, we go to the upper table we find that for Burton's oils the oil which comes nearest to this 165 seconds is next to the last oil with 158 seconds, which has a gravity of only 12 Baume; in other words, very much more different from the oils from California crude than the latter are from mid-continent crude—a very marked difference in the gravity corresponding to certain viscosities.

In other words, we can express Burton's unique relationship in either one of two ways: his oils are heavier for a given viscosity, or they are less viscous for a given gravity.

In order to make more clear his unique relationship between gravity and viscosity, Burton adopted the common expedient of making a plot which plotted gravity as one scale and viscosity as another scale.

This is described beginning in line 107 of page 2 of the patent, where I read:

"For the purpose of best defining this relation between gravity and viscosity which distinguishes my product, I find it convenient to plot a gravity-viscosity curve in rectangular coordinates. This curve I find to be in general form a conic section."

And then he proceeds to give the approximate mathematical equation, which shows this curve.

Q. Now to make Burton's exact meaning clear to the Court, I wish you would re-plot his data as given in the table, beginning on line 1 of page 2, where he gives the actual gravities and actual viscosities of nineteen oils belonging to his new series and of eleven ordinary oils.

A. I have prepared a plot in this manner, which I now produce. It also contains other points than those which you specified and those I will mention later. This plotting method, of course, gives a much clearer picture than either a table or a mathematical equation, and it is the step which Burton himself took in order to make clear his unique relationships. Of course, either the tables or the mathematical equation will on sufficient study give the same results, but it is much easier to grasp if we follow Burton's suggestion and make a plot.

I have here a plot in rectangular coordinates, just as Burton mentions, and I have plotted on it mainly the references and the particular results which Burton himself gives. We have for the vertical scale the viscosity in seconds Saybolt, our scale varying from twenty-five seconds at the bottom to 450 seconds at the top, in order to cover all of the oils which Burton mentions in his patent. The gravity scale covers from ten Baume up to forty-five Baume, although Burton's own results only go up to thirty Baume, but some of the references cited are a little over forty-five gravity, so the chart has been extended to show those.